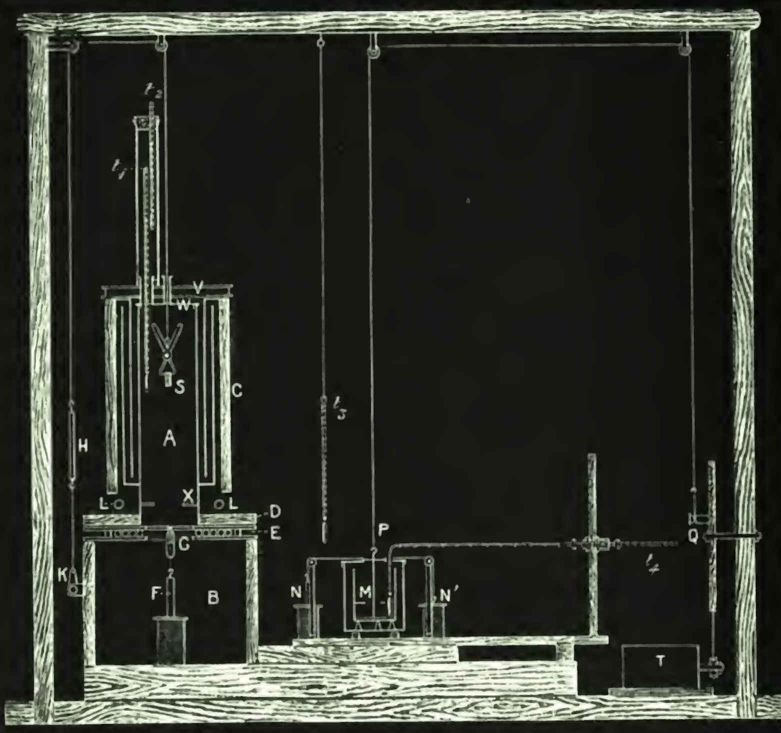

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OF THE

ROYAL SOCIETY OF LONDON.

From November 19, 1885, *to December* 17, 1885.

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THE ROYAL SOCIETY.

"On the Atomic Weight of Glucinum (Beryllium)." Second Paper. By T. S. Humpidge, Ph.D., B.Sc., Professor of Chemistry in the University College of Wales, Aberystwyth. Communicated by E. Frankland, F.R.S. Received February 27, 1885. Read March 5.

In a former communication which I had the honour of making to the Royal Society,* I described a method of preparing metallic glucinum and of determining its specific heat. From my experiments I deduced the result that the atomic weight of the metal must be 13·6 (*circâ*) in order to agree with Dulong and Petit's rule.

It is well known that the position assigned to glucinum in the periodic arrangement of the elements requires an atomic weight of two-thirds the above number, or approximately 9, and that with the larger atomic weight it falls between carbon and nitrogen, and is entirely out of place.

Various criticisms were offered to explain this apparently anomalous result. To the suggestion of Professor J. E. Reynolds† that the pure metal would have a specific heat 50 per cent. greater than that of a sample containing 6 per cent. of impurities, I have already replied.‡ Another explanation was offered by Brauner,§ who thought it possible that the specific heat of the metal might increase with the temperature and thus agree with Dulong and Petit's rule, at some higher temperature-interval than 0—100°. Brauner's suggestion was based upon the position which glucinum should occupy in the periodic arrangement and which would be similar to that of boron and carbon. Some determinations of the specific heat of the metal up

* "Phil. Trans.," 1883, Part II, p. 601.
† "Proc. Roy. Soc.," vol. 35, p. 248.
‡ "Proc. Roy. Soc.," vol. 35, p. 358.
§ "Berlin Ber.," xi, 872.

to 300° made by Nilson and Petterson* confirmed this prediction to a certain extent, but as this important question could not be considered as definitely decided, the following investigations were undertaken. Owing to the small amount of time at my disposal the experiments have absorbed the greater part of my leisure for the past fourteen months, and in the mean time my results have been partly forestalled by other investigators, but the importance of the subject justifies me in publishing what is little more than a confirmation of the work of others.

My experiments may be divided into two groups, (i) those on the specific heat of pure glucinum at varying temperatures, and (ii) those on the vapour-density of volatile glucinum compounds.†

I. *Specific Heat of Metallic Glucinum at varying Temperatures.*

For the determinations of the specific heat of glucinum at varying temperatures a fresh quantity of the metal was prepared from the pure chloride by the method which I have previously described (*loc. cit.*). The chloride was obtained from oxide which had been carefully purified with ammonium carbonate. Experiments in which impure glucina was fused with acid potassium fluoride, and the fused mass extracted with water containing hydrofluoric acid, proved that if iron is present much of it goes into solution with the glucinum. The usual method with ammonium carbonate gives better results, especially if the process is several times repeated, and if at last an insufficient quantity of the solvent is used, so that some of the glucina remains undissolved with the last traces of the alumina. The concentrated solution of the double carbonate‡ can then be easily decomposed by leading steam into it through a wide tube. The dangerous bumping which always happens when the concentrated solution is boiled is thus completely avoided. In this way, after three or four purifications, it is easy to obtain a sample of glucina which when fused with acid potassium fluoride dissolves in water containing hydrofluoric acid without leaving the slightest trace. A very convenient form of charcoal for mixing with the oxide to prepare the chloride is a kind of lamp-black known in trade as *gas-black.* When burnt it leaves a much smaller quantity of ash than the best sugar charcoal, and can be mixed with the oxide much more easily and intimately than the latter.

Most of the metallic glucinum prepared from this pure chloride was in the form of thin, lustrous, highly crystalline laminæ. The purer portions were selected and compressed in a steel mortar to a compact, lustrous cylinder 11 mm. long and 8 mm. in diameter.

* "Berlin Ber.," xiii, 1456.

† The latter experiments were done before the former.

‡ See Appendix A.

An analysis of a portion of this metal after the specific heat determinations had been done was conducted as follows. Since aluminium was absent in the oxide and could not have been introduced during the preparation of the metal, the only impurities to be looked for were silicon, iron, and glucinum oxide. On solution of the metal in a dilute acid, the silicon would mostly remain behind in the free state, but a part would be liberated as silicon hydride.* The oxide would also remain undissolved as the metal had been heated up to 450° in the specific heat determinations. The iron would, of course, dissolve.

29·4 mgrms. of the metal was dissolved in dilute sulphuric acid, and left a minute, imponderable residue, which was estimated at 0·2 mgrm. It remained floating in the liquid for some time, was light coloured, and was probably glucina mixed with some ferric oxide. The iron in solution was estimated by the depth of colour produced by ammonium sulphocyanate, just as by the operation known as "Nesslerising" minute quantities of ammonia are estimated. I believe this method gives more accurate results for the estimation of very small quantities of iron than the usual process with potassium permanganate. In using a dilute solution of this reagent a considerable excess must be added before the pink colour becomes visible, and the end point or amount of excess is very difficult to determine; whereas with ammonium sulphocyanate and working under similar conditions it is very easy to match the two colours and to determine accurately a minute quantity of iron. The iron in the above solution of glucinum sulphate was oxidised by a drop of weak bromine-water, the excess of bromine boiled off, and a given volume of ammonium sulphocyanate added in a Nessler cylinder. To another similar cylinder, containing the same quantity of free acid and ammonium sulphocyanate and made up to the same volume, was added a solution of ferric sulphate containing 0·1 mgrm. iron per c.c. Of this solution 0·3 c.c. was required, corresponding to 0·03 mgrm. iron. The composition of the metal was therefore :—

Glucinum	99·20
Glucina	0·70
Iron	0·10
	100·00

in other words, it was almost pure. Its purity was also shown by the difficulty with which it dissolved in dilute acids. A small piece which had been left in dilute sulphuric acid (1 : 10) over night was only partly dissolved in the morning, but solution took place rapidly on warming.

* The silicon in commercial magnesium behaves in this manner.

Several attempts were made to determine the density of this block of metal. They were all unsuccessful, owing to the difficulty of completely removing the enclosed air.

Another sample was therefore used in the loose form as extracted from the iron boats. The following results were obtained :—

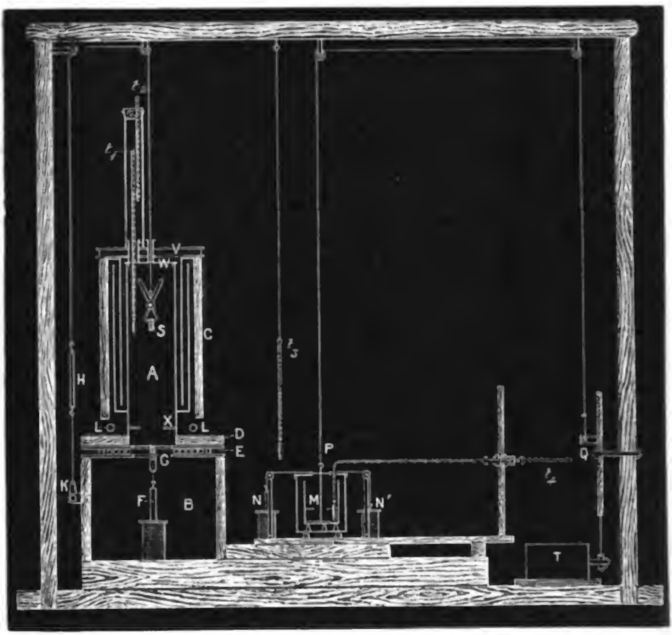
$$d\frac{20}{20} = 1{\cdot}88 \text{ (i)},\ 1{\cdot}90 \text{ (ii)};\ \text{mean} = 1{\cdot}89.$$

The composition of this metal was—

Gl	99·00
GlO..............	0·43
Fe	0·57
	100·00

and making allowance for these impurities the true density of glucinum becomes $d\frac{20}{20}=1{\cdot}85$.

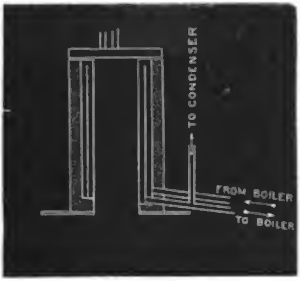
Fig. 1.



Owing to the small amount of substance used (about 59 mgrms.), the result is only accurate to the second decimal place.

The calorimeter employed was an improved form of the instrument described in my former paper (*loc. cit.*). The opening and closing of the two sliders, and the movement of the catch to release the substance, were, however, made automatic by suitable electrical connexions. It was thus only necessary to run the calorimeter under the heater and back again in order to transfer the substance from the latter to the former. The calorimeter (M, fig. 1), with its brass casing, and the bent thermometer (t_4), were the same as those used before; the agitator (P) was worked by a small electromotor (T). Two new heaters were used, one for temperatures up to 150°, and one for temperatures from 150°

FIG. 2.



to 300°. The former (fig. 2) consists of a double brass vessel with a thick jacket of slag-wool. The vapour of the boiling liquid passes from the boiler to the heater by one tube, and leaves by another, is condensed and flows back to the boiler. The liquids used in this heater were methyl alcohol, water, and xylol, but the results obtained with the first-named liquid were inaccurate owing to the small rise (0·6°) in the temperature of the calorimeter. In this apparatus it is possible to obtain a temperature constant to $\frac{1}{10}$° for any length of time.*

The heater for high temperatures (A, fig. 1) was made on the pattern described by Lothar Meyer†, the construction of which will be evident from the figure. C is an outer non-conducting casing, L is a ring burner, from which the heated gases pass up through the heater and escape through perforations in the double lid (V). This heater is mounted on a thick slab of asbestos (D), by means of which it stands on two brass plates (E), which are separated from one another by a coil of thin lead tubing, through which a current

* This is only true for a liquid with a constant boiling point; the xylol used had to be fractionated several times before a constant temperature could be obtained.

† "Berlin Ber.," xvi, 1087.

of cold water flows. In this way the chamber (B) into which the calorimeter has to be run is kept quite cool. A double tin screen (not shown in the figure) serves also to prevent the calorimeter from receiving extraneous heat. The whole operation of transferring the substance from the heater to the calorimeter can be performed in less than two seconds, and the gain of heat by the calorimeter during this period cannot be measured. In a series of blank experiments it was found that when the calorimeter remained under the heater (at 300°) for 15 seconds, the thermometer t_4 rose $\frac{1}{100}$°. The temperature of the substance in the heater was measured by three delicate thermometers from Geissler. Their fixed points were determined in ice, water, naphthalene, and benzophenone, but they were not calibrated. The gas supply for this heater was regulated by three regulators made by Giroud, of Paris, burning 60, 80, and 110 litres of gas per hour, and giving temperatures of 200°, 240°, and 310° respectively. With these regulators the temperature could be kept constant to about $\frac{1}{4}$°, provided there were no great fluctuations in the gas pressure. I have sadly felt the need of a good regulator for high temperatures.* The suspension of the substance (S) in the heater was by a small pair of tongs, made so that the jaws open when the arms are closed. The tongs with the substance are supported by a thin platinum wire which is attached, outside the heater, to a thread passing over pulleys and fastened to a catch (K). When this catch is released the tongs fall by their own weight until they reach the ring X; this brings the arms closer together, and the substance drops into the calorimeter. The connexions are so arranged that when the calorimeter is run under the heater, the lid of the calorimeter casing is first opened, then the slider under the heater, and finally the catch is released, and the substance falls. As the calorimeter returns the two sliders are again closed.

The liquid used in the calorimeter was purified turpentine, of which the specific was determined as follows :—

(i.) By the method of mixtures, using metallic magnesium, of which the specific had been found to be $c_{100}^{13}=0{\cdot}2442$. This gave for the turpentine $k_{10}=0{\cdot}4146$.†

(ii.) By Andrews' calorifier‡ the mean of two series of experiments gave $k_{10}=0{\cdot}4103$.

(iii.) By Pfaundler's apparatus§ (Joule's principle), in which equal weights of water and turpentine contained in two similar calorimeters are heated by two platinum wires of equal resistance, through which

* I am much indebted to Professor L. Meyer for advice and assistance in the construction of these two heaters.

† For details of these and the following experiments, see Appendix C.

‡ "Ann. Chim. et Phys." [3], xiv, 92.

§ "Wien. Akad. Ber.," lix.

the same current passes. Three experiments gave the mean result, $k_{10}=0{\cdot}4085$.

The general mean of these experiments is $k_{10}=0{\cdot}4112$.

The specific heat of water was taken as 1·0000 at 0°, and the correction for temperature used was that obtained by Münchhausen and Wüllner, and by Baumgartner and Pfaundler, viz., $k_t=1+0{\cdot}0003t$. Regnault's expression for the change in the specific heat of turpentine was employed, *i.e.*, $k_t=k_0+0{\cdot}00124t$, omitting the third term, which only inappreciably affects the results in the small range through which the liquid was heated.

The following table contains the results of the specific heat determinations of glucinum* made with this apparatus. Column I gives the results actually obtained; Column II these results corrected for the impurities contained in the metal:—

		I.	II.
Experiments 1 and 2	c^{11}_{100} =	0·4267	0·4286
„ 3 „ 4	c^{13}_{145} =	0·4500	0·4515
„ 5 „ 6	c^{11}_{195} =	0·4676	0·4696
„ 7 „ 8	c^{15}_{240} =	0·4866	0·4885
„ 9 „ 10	c^{14}_{312} =	0·5087	0·5105

These numbers show a rapid and continuous increase in the specific heat as the temperature rises. In order to find whether this increase continued above 300°, two experiments were made at higher temperatures with a double calorimeter. This calorimeter consisted of two thin brass vessels, placed close together with suitable thermometers and agitators. One of these received the glucinum, and the other a platinum cylinder of known weight. These two substances were placed in two similar glass tubes, packed side by side in a wide iron tube with slag-wool, and the whole heated in the furnace which was used for the vapour-density determinations.† After the heating had been continued for about three hours, the iron tube was withdrawn, and the substances quickly tipped into the calorimeters. In calculating the temperature, Violle's results of the specific heat of platinum at varying temperatures were used. These two experiments gave th following results:—

		I.	II.
Experiment 11	c^{11}_{300} =	0·5178	0·5199
„ 12	c^{17}_{447} =	0·5384	0·5403

which show that although the specific heat continues to increase, it does so more slowly than at lower temperatures.

* The metal was enclosed in a platinum capsule, soldered air-tight with pure gold

† See Sequel.

The quantity of heat (Q_t) required to raise 1 gram of a substance from 0° to t° can be expressed by the equation—

$$Q_t = kt + \alpha t^2 + \beta t^3,$$

where k is the true specific heat at 0°, and α and β are constants. From this equation the mean specific heat between any two temperatures (t' and t) is

$$c_{t'}^{t} = k + \alpha(t + t') + \beta(t^2 + tt' + t'^2).$$

The numerical values obtained for the three unknowns from the experimental values of $c_{t'}^{t}$ are

$$k = 0{\cdot}3756; \quad \alpha = 0{\cdot}00053; \quad \beta = -0{\cdot}00000038.$$

A comparison of the value for the mean specific heats calculated from the above expression with those obtained by direct experiment shows that the two agree within the experimental errors, and that this expression correctly represents the change in the specific heat of glucinum.

$t' : t$.	$c_{t'}^{t}$ found.	$c_{t'}^{t}$ calculated.
11° : 100°	0·4286	0·4302
13 : 145	0·4515	0·4505
11 : 193	0·4696	0·4687
15 : 240	0·4885	0·4875
14 : 312	0·5105	0·5097
11 : 360	0·5199	0·5215
17 : 447	0·5403	0·5425

The three constants being known, the true specific heat at any temperature (k_t) is found by making $t'=t$, and the above equation becomes

$$k_t = k + 2\alpha t + 3\beta t^2,$$

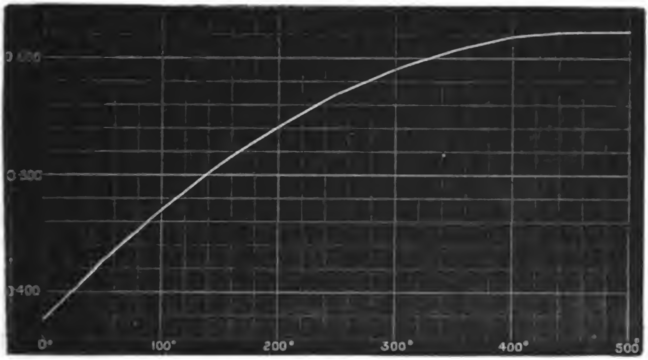
or with numerical values,

$$k_t = 0{\cdot}3756 + 0{\cdot}00106t - 0{\cdot}00000114t^2,$$

whence the following values for k_t may be calculated—

$$\begin{aligned} k_0 &= 0{\cdot}3756 \\ k_{100} &= 0{\cdot}4702 \\ k_{200} &= 0{\cdot}5420 \\ k_{300} &= 0{\cdot}5910 \\ k_{400} &= 0{\cdot}6172 \\ k_{500} &= 0{\cdot}6206 \end{aligned}$$

FIG. 3.



Curve showing specific heat of glucinum at varying temperatures.

A curve showing the true specific heat at varying temperatures is given in fig. 3. According to the above expression the specific heat reaches a maximum at about 470°, and then falls. But it is doubtful whether this expression would correctly represent the specific heat at temperatures above 500°. Weber's experiments with diamond and graphite tend to confirm this doubt. His results between $-79°$ and $+280°$ show a rapid increase in the specific heat, and give a parabolic curve similar to that obtained for glucinum. But in the other experiments which he made between 600° and 1000° the increase in the specific heat is much less rapid, and the curve takes another form. However this may be, my results show that the specific heat of glucinum rapidly increases up to about 400°, and that between 400° and 500° it remains practically constant at the approximate value of 0·62. If this number is multiplied by the atomic weight taken as 9·1, the atomic heat becomes 5·64. It is therefore clear that this number represents the true atomic weight, and not 13·6, as was previously deduced from the specific heat between 100° and 10°. Glucinum is thus to be classed with those elements of which the specific heat increases rapidly with the temperature, and, like most elements with low atomic weights, its atomic heat is considerably below the average.

The expression for the true specific heat of the metal given by Nilson's results is similar to that obtained from mine. His values for the constants, as calculated by me, are: $k_0 = 0·3710$, $\alpha = 0·00058$, and $\beta = -0·00000044$, which closely agree with those derived from my experiments.

II. *Vapour-density of Volatile Glucinum Compounds.*

The compounds of glucinum which can be volatilised unchanged are those which it forms with organic radicals and with the halogens. Of the former class Cahours states* that he has prepared the ethide and propide, and gives their boiling-points, but all my efforts to prepare either of these compounds in a sufficiently pure state to determine its vapour-density were fruitless. I operated, as Cahours recommends, on an excess of metallic glucinum with mercuric ethide or propide in sealed tubes; but either the tubes exploded violently, always when a temperature of 135° was exceeded, or if the change was successfully accomplished, as shown by the separation of metallic mercury, the compound decomposed again on distilling. Mercuric ethide acts on glucinum chloride, slowly at 100°, more rapidly at 130°, and a considerable quantity of mercury ethyl chloride is produced, which partly remains dissolved in the excess of mercuric ethide, and partly separates out in the usual pearly scales. A similar reaction takes place between mercuric ethide and glucinum bromide, but from neither of these reactions could a volatile compound of glucinum be obtained. I hope to be able to examine these interesting reactions at a later date.

As the organic compounds of glucinum were not available for vapour-density determinations, my attention was directed to the halogen compounds. The bromide, chloride, and iodide of glucinum are all volatile, and their volatility is in the order named. The first two volatilise without decomposition if water and air are absent; the last-named decomposes on heating. In the presence of a trace of air, both the chloride and bromide decompose when heated, the latter more easily than the former; in both cases the haloid element is set free. And since both these compounds corrode glass and porcelain when vaporised in vessels of these substances, the determination of their vapour-density is a matter of some difficulty.

I first made some determinations in glass tubes with the chloride, using Schwarz's modification† of V. Meyer's displacement method. The glass was corroded, and the results were not concordant. Porcelain was also attacked. The tubes nearly always contained free chlorine, although the experiments were done in pure dry nitrogen. It was then decided to try platinum for the determination, and after it had been found that glucinum chloride could be sublimed unchanged

* "Compt. rend.," lxxvi.

† "Berlin Ber.," xvi, 1051.

The obvious errors in this modification have been pointed out by V. Meyer ("Berlin Ber.," xvii, 1334). They are chiefly that a portion of the wide tube is irregularly heated, and that the large boat which carries the substance takes air with it in its fall into the tube.

in a platinum tube in a stream of nitrogen, a platinum vessel was obtained from Messrs. Johnson, Matthey, and Co. This platinum vessel was of the usual size and dimensions of those employed for vapour-density determinations by displaced air, except that the narrow tube was about one-half the ordinary length. The open end of this tube was firmly attached to the upper part of the usual glass apparatus by well-wired caoutchouc tubing.* The substance was introduced, and the displaced air measured by the method used by Meier and Crafts† in their researches on the vapour-density of iodine. As a graduated vessel for the reception of the displaced air, an ordinary calibrated burette was used, the lower end being connected with a tube of the same width by strong caoutchouc tubing. These two tubes were held in common retort-stand clips, and could be easily adjusted, so that the mercury was at the same level in each. They were connected with the vessel in the furnace by a narrow glass tube (1 mm. bore), and the one containing the air was immersed in a vessel of water. As a furnace, two of Fletcher's draft furnaces were used, placed one above the other, with a wide clay cylinder between them. This gave a chamber 30 cm. high and 12·5 cm. in diameter, which could be heated up to any temperature from about 400° to 800°. The temperature could be kept sufficiently constant for these experiments with a good gas tap.

The platinum vessel was freely suspended in two clay crucibles, placed mouth to mouth, and with a hole knocked in the bottom of the upper. This served to protect the vessel from the direct action of the furnace gases. The temperature was measured by a Siemens' pyrometer, the porcelain tube of which was connected with platinum wires to the platinum vessel.‡

With this apparatus the vapour-densities of glucinum chloride and bromide were determined. Glucinum chloride was prepared in several ways, but the best method was found to be that recommended by Nilson, which consists in heating a few mgrms. of the metal in a stream of dry hydrochloric acid in a narrow platinum tube. The chloride is sublimed as near the end of the tube as possible, the connexions removed, and a small cap fitted on the open end. The tube is then cut off with a pair of scissors, so as to form a small capsule inclosing the chloride. This is introduced into a well-corked glass tube and weighed. While the preparation of the chloride was going on, the

* During the progress of this part of my work, I received the account of Nilson and Pettersson's experiments on the vapour-density of glucinum chloride ("Berlin Ber.," xvii, 987). It will be observed from the sequel that I have adopted some details from them, notably the use of carbonic acid for displacement.

† " "Berlin Ber.," xiii, 851.

‡ For a description of the construction and graduation of this instrument, see Appendix B.

apparatus was gently heated, and dry carbonic acid passed through it by a glass tube going to the bottom of the platinum vessel, until the issuing gas was completely absorbed by caustic potash.* The glass tube was then gradually withdrawn, and the temperature raised. As soon as the temperature had become constant, which was easily seen by the mercury remaining stationary in both tubes and the resistance of the pyrometer being constant, the cork at the top of the vessel was removed for an instant, the small platinum tube with the substance dropped in, and the cork immediately replaced. The equilibrium was thus hardly disturbed, and a few minutes afterwards the substance was allowed to fall into the heated vessel.

In the two following determinations (the only ones which were made) evaporation commenced at once, and was complete in about twenty seconds. The following are the data of these two experiments:—

Exp.	Substance.	Displaced CO_2.	*t.*	*d.*
1	26·4 mgrms.	7·47 c.c.	635°	2·733
2	28·0 ,,	7·98 ,,	785	2·714
		Mean		2·724

After each experiment the platinum vessel was washed out with a mixture of potassium iodide and starch, but no trace of free chlorine was found. The small platinum tube was then washed with water, dried, and weighed; this gave the weight of the substance used.

The vapour-density of glucinum bromide was next determined in the same apparatus. This substance can be obtained like the chloride, by heating the metal in dry hydrobromic acid, but, owing to the difficulty of obtaining a regular stream of the dry gas, it is best prepared by burning the metal in gaseous bromine. Combination between the two elements takes place at a low red heat, with brilliant incandescence. The crude bromide is afterwards purified by resubliming it in a current of hydrogen or carbonic acid. Like the chloride, it can be sublimed unchanged in carbonic acid. Glucinum bromide sublimes at a lower temperature than the chloride, and, unlike the latter compound, vaporises before it melts. It begins to volatilise at about 450°. When carefully sublimed at a low temperature, it forms beautiful snow-white silky needles, and does not attack glass if air and moisture are perfectly absent. If heated in the presence of air, free bromine is at once produced.

A quantity of the crude substance was prepared by heating a few

* There was always a minute bubble of air left unabsorbed by the potash, even when the gas was passed through the apparatus for 20 hours. It was probably due to diffusion through the caoutchouc connexions, and did not appear to influence the results.

centigrams of metallic glucinum in bromine vapour. This was purified by sublimation in dry hydrogen, and was finally sublimed into several narrow glass tubes, each of which was sealed up. When a determination was to be made, the two ends of one of these tubes were cut off, and the solid lump of the bromide quickly rammed into a small platinum tube closed at one end. A cap was fitted on this tube, and it was weighed in the same manner as the chloride.

The vapour-density was determined like that of the chloride, and the following are the data of the four experiments made:—

Expt.	Substance.	Volume of CO_2.	*t.*	*d.*
1	—	—	440°	evaporation very slow.
2	35·9 mgrms.	4·28 c.c.	608	6·487
3	61·1 „	7·53 „	630	6·276
4	26·0 „	3·22 „	606	6·245
			Mean	6·336

In these experiments there was always a trace of free bromine produced. In experiment 4, where it was largest, the iodine, set free on the addition of potassium iodide, was estimated by centinormal potassium arsenite, of which 0·2 c.c. was required. This corresponds to 0·16 mgrm. bromine, or less than 1 per cent. of the quantity present. The decomposition which had taken place could not, therefore, affect the result to any appreciable extent, and that the substance had completely evaporated was proved by the fact that the small platinum tube weighed the same before and after the experiment within one or two tenths of a milligram.

The possible densities for glucinum chloride and bromide are—

For $Gl''Cl_2$	2·76	For $Gl'''Cl_3$	4·14, and
„ $Gl''Br_2$	5·84	„ $Gl'''Br_3$	8·76

The values found were—

For $GlCl_2$ ...... 2·72, and for $GlBr_2$ ...... 6·34.

It is therefore evident that the molecules of these two substances in the gaseous state are represented by the formulæ $GlCl_2$ and $GlBr_2$ respectively, in which the metal is a dyad, and has the atomic weight 9·1. Thus from Avogadro's law, to which there are no known exceptions, the conclusion is confirmed which was obtained from the specific heat of the element at high temperatures.

The long disputed question of the atomic weight of glucinum is thus definitely and finally decided in favour of that number which satisfies the requirements of the periodic law, and another element is added to the long list of those whose atomic weights have been cor-

rected by this important generalisation. In all future determinations of the atomic weight of an element, the position which the element should occupy in the periodic arrangement must receive due importance, and had I fully recognised this two years ago, I should perhaps have stated my conclusions and criticisms less positively than I did.

Appendix A.

On the Composition of the Double Carbonate of Glucinum and Ammonium.

An analysis of this important compound has been published by Debray,* but the formula he obtained being somewhat complex, I have prepared and analysed the pure substance. A strong solution of ammonium carbonate was digested with moist glucinum carbonate at a gentle heat until saturated. The clear solution was then heated until it became cloudy, and any excess of ammonium carbonate decomposed, then again filtered and mixed with its own bulk of strong alcohol. The crystals, which only separated slowly and adhered firmly to the walls of the containing vessel, were drained, washed with alcohol, and dried between filter-paper.

The analyses of two separate specimens gave the following results:—

	I.	II.	Mean.	Calculated.
BeO	18·77	19·22	19·00	18·40
CO_2	42·13	43·16	42·65	43·00
$(NH_4)_2O$	26·39	26·25	26·31	25·41
H_2O (diff.) ..	12·71	11·37	12·04	13·19
	100·00	100·00	100·00	100·00

which correspond to the formula $2(GlCO_3,Am_2CO_3),Gl(OH)_2+2H_2O$. This resembles the formula obtained by Debray, viz.,

$$3(GlCO_3,Am_2CO_3),Gl(OH)_2,$$

but contains more water. Both of my specimens were dry crystalline powders, without a trace of adhering alcohol, and with a faint odour of ammonia. Specimen II had been kept for several weeks in a stoppered bottle.

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* "Ann. d. Chim." [3], xliv, 5.

Appendix B.

Note on Siemens' Pyrometer.

The following are the details of construction and graduation of the pyrometer employed in the vapour-density determinations. About 100 cm. of thin platinum wire (0·13 mm. diameter) was coiled round a thick piece of ordinary clay tobacco-pipe, and three thicker platinum wires were soldered with gold to the two ends. To these thicker wires were soldered three copper wires, which went to the bridge where the resistance was measured. The whole was enclosed in a porcelain tube, one end of which had been closed by a plug of clay, and the wires were so arranged that the junctions between the copper and platinum came at the same height, just within the tube. By suitably arranging the three wires and a known resistance in the arms of the bridge, measurements could be made of the resistance of the thin coil without that of the leads, so that any change in resistance was due to the altered resistance of the coil alone. The wire of which the coil was made had previously been heated several times to redness.

In order to graduate the instrument it was immersed in melting ice, boiling water, aniline, diphenylamine, sulphur, selenium, and zinc. For boiling these substances (with the exception of water) iron tubes welded together at one end, of 5 cm. diameter and 46 cm. length, were used. The upper portion of the tube for about 8 cm. was surrounded with a coil of thin lead tubing, through which a stream of cold water was allowed to flow. This acted as a very efficient condenser, and kept the upper part of the tube cool enough to handle easily. The results obtained are expressed in the following table, in which the first column gives the substance, the second its melting or boiling point, and the third the actual resistance found. In the fourth column the resistances are reduced to that at 0°=1, and in the fifth are given the corresponding resistances, calculated from the empirical formula—

$$r_t = 1 + 0\cdot0027t - 0\cdot00000019t^2.$$

Substance.	t.	r_t.	$r_0 = 1$.	Calculated.
Ice	0°	13·42	1·000	1·000
Water	100	17·02	1·268	1·268
Aniline	184	20·08	1·496	1·493
Diphenylamine..	310	24·43	1·819	1·819
Sulphur........	448	29·14	2·171	2·172
Selenium	665	36·08	2·688	2·712
Zinc	940	45·38	3·381	3·370

The agreement between the calculated and observed results is good, except in the last two instances. The selenium probably contained some sulphur, which would lower its boiling point, and only one experiment was made with zinc, as this temperature could only be reached in the furnace used under an exceptionally favourable gas pressure.

The change in the resistance of commercial platinum has been investigated by the late Sir W. Siemens, M. Benoit, and others.

Siemens expressed his results in the form—

$$r_t = r_o(\mathrm{AT}^{\frac{1}{2}} + \mathrm{BT} - \mathrm{C}),$$

and this can be thrown into a general expression similar to that which I have used, viz.:—

$$r_t = r_o(1 + \alpha t - \beta t^2).$$

Benoit's results* can also be expressed by a similar formula.

But although there is a general agreement between these different results, the values of the coefficients vary considerably, and it is therefore always necessary in using this pyrometer to calibrate it for every wire. I have even found that the change of resistance of two pieces of wire from the same bobbin varied considerably.

M. Benoit has kindly furnished me with the following synopsis of various results, which, although they differ much from one another, can all be expressed by the general formula given above:—

	Siemens.					
t.	I.	II.	III.	Benoit.	Erhardt.†	Humpidge.
0° ..	1·00	1·00	1·00	1·00	1·00	1·00
100 ..	1·29	1·25	1·31	1·23	1·23	1·27
200 ..	1·47	1·50	1·62	1·46	1·46	1·53
400 ..	1·84	1·98	2·23	1·91	1·90	2·05
600 ..	2·16	2·48	2·81	2·34	2·31	2·55
800 ..	2·43	2·97	3·37	2·75	2·69	3·04

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* *Compt. rend.*, lxxvi, 342. The formula is incorrectly given in Wiedemann's "Electricität," the sign of second coefficient being positive instead of negative. One of the temperatures used in this research was that of boiling cadmium, which was taken from the earlier and incorrect determinations (with an iodine thermometer) of Deville and Troost as 860° instead of *circâ* 770°. M. Benoit has, however, informed me, in a private communication, that on introducing this correction the general form of the equation remains the same.

† "Wied. Ann." xxiv, 215.

Appendix C.

Details of Specific Heat Determinations.

I. *Specific Heat of Turpentine.*

(i.) ***By Method of Mixture with Magnesium.***

(*a.*) *Specific Heat of Magnesium.*

w=weight of substance, w'=weight of platinum wire as support, W=weight of water; water-equivalent of calorimeter, agitator, and thermometer=2·84, T=temperature of substance, t=initial temperature of liquid, θ=final temperature of mixture, c=calculated specific heat. [Specific heat of water at $t°=1+0·0003t$.]

Expt. 1.. $w = 4·739$, $w' = 0·156$, $W = 85·21$,
$T = 99°·61$, $\theta = 12°·57$, $t = 11°·41$
$\therefore c_1 = 0·2474$.

Expt. 2.. $W = 85·22$, $T = 99°·96$, $\theta = 11°·38$, $t = 10°·21$
$\therefore c_2 = 0·2452$.

Expt. 3.. $W = 85·22$, $T = 98°·80$, $\theta = 12°·79$, $t = 11°·65$
$\therefore c_3 = 0·2428$.

Expt. 4.. $W = 85·22$, $T = 98°·78$, $\theta = 13°·78$, $t = 12°·66$
$\therefore c_4 = 0·2412$.

Mean: $c_{14}^{100} = 0·2442$.

(*b.*) *Specific Heat of Turpentine.*

w and w' as before. W=weight of turpentine, equivalent of calorimeter, &c.=2·83.

Expt. 1.. $W = 75·80$, $T = 98°·20$, $\theta = 14°·57$, $t = 11°·67$,
$\therefore c_1 = 0·4111$.

Expt. 2.. $W = 75·17$, $T = 99°·54$, $\theta = 15°·27$, $t = 12°·47$,
$\therefore c_2 = 0·4274$.

Expt. 3.. $W = 75·12$, $T = 98°·20$, $\theta = 14°·60$, $t = 11°·74$,
$\therefore c_3 = 0·4206$.

Expt. 4.. $W = 75·43$, $T = 97°·95$, $\theta = 12°·97$, $t = 10°·03$,
$\therefore c_4 = 0·4140$.

Expt. 5.. $W = 75·02$, $T = 97°·98$, $\theta = 13°·58$, $t = 10°·64$,
$\therefore c_5 = 0·4131$.

Expt. 6.. W = 75·09. T = $99°·26$, $\theta = 14°·50$, $t = 11°·62$,
$\therefore c_6 = 0·4174$.

Expt. 7.. W = 76·24. T = $99°·50$, $\theta = 15°·09$, $t = 12°·28$,
$\therefore c_7 = 0·4210$.

Mean : $c_{10}^{15·1} = k_{12·7} = 0·4179$,
and $k_{10} = 0·4146$.

(ii.) *By Andrews' Calorifer.*

Equivalent of calorimeter and thermometer=10·33, weight of water=262·34. The mean of five experiments gave $\theta=8°·436$, $t=7·016°$,
whence equivalent of calorifer=

$$= 272·67 \times 1·42 = 387·19 \div 1·0023$$
$$= 386·20$$

In two series of experiments, using turpentine instead of water, the following results were obtained :—

Series i.—Weight of turpentine=229·26,

$$\theta = 12°·298, \; t = 8°·594 \text{ (5 experiments)}$$
$$\therefore k_{10·5} = 0·4107.$$

Series ii.—Weight of turpentine=229·26,

$$\theta = 12°·480, \; t = 8°·790 \text{ (4 experiments)}$$
$$\therefore k_{10·5} = 0·4115,$$

whence $k_{10} = 0·4105$.

(iii.) *By Electrical Method.*

Mean of three experiments gave

$k_{16·2} = 0·4180$, compared with water at $14°·2$,
whence $k_{10} = 0·4085$.

The general mean of the three sets of determinations is

$$k_{10} = 0·4112,$$

which was adopted, with a correction for the temperature, in calculating the specific heat of glucinum in the following experiments :—

II. *Specific Heat of Glucinum.*

(i.) *In Turpentine.*

Weight of substance=0·745, weight of platinum casing=1·527, water equivalent of calorimeter, &c.=2·83.

Expt. 1.. $W = 74{\cdot}98,\ T = 99^\circ{\cdot}45,\ \theta = 11^\circ{\cdot}29,\ t = 10^\circ{\cdot}33,$

$\therefore c_1 = 0{\cdot}4272.$

Expt. 2.. $W = 74{\cdot}98,\ T = 99^\circ{\cdot}36,\ \theta = 11^\circ{\cdot}08,\ t = 10^\circ{\cdot}12,$

$\therefore c_2 = 0{\cdot}4262,$

and $c^{11\cdot2}_{99\cdot4} = 0{\cdot}4267.$

Expt. 3.. $W = 74{\cdot}97,\ T = 145^\circ{\cdot}2,\ \theta = 11^\circ{\cdot}95,\ t = 10^\circ{\cdot}44,$

$\therefore c_1 = 0{\cdot}4481.$

Expt. 4.. $W = 74{\cdot}97,\ T = 145^\circ{\cdot}2,\ \theta = 14^\circ{\cdot}63,\ t = 13^\circ{\cdot}15,$

$\therefore c_2 = 0{\cdot}4518,$

and $c^{13\cdot8}_{145\cdot2} = 0{\cdot}4500.$

Expt. 5.. $W = 74{\cdot}87,\ T = 190^\circ{\cdot}1,\ \theta = 10^\circ{\cdot}08,\ t = 7^\circ{\cdot}95,$

$\therefore c_1 = 0{\cdot}4668.$

Expt. 6.. $W = 74{\cdot}87,\ T = 196^\circ{\cdot}2,\ \theta = 11^\circ{\cdot}18,\ t = 8^\circ{\cdot}99,$

$\therefore c_2 = 0{\cdot}4684,$

and $c^{10\cdot6}_{193\cdot1} = 0{\cdot}4676.$

Expt. 7.. $W = 74{\cdot}97,\ T = 241^\circ{\cdot}9,\ \theta = 14^\circ{\cdot}58,\ t = 11^\circ{\cdot}82,$

$\therefore c_1 = 0{\cdot}4875.$

Expt. 8.. $W = 74{\cdot}97,\ T = 236^\circ{\cdot}2,\ \theta = 16^\circ{\cdot}23,\ t = 13^\circ{\cdot}58,$

$\therefore c_2 = 0{\cdot}4856,$

and $c^{15\cdot4}_{239\cdot6} = 0{\cdot}4866.$

Expt. 9.. $W = 74{\cdot}97,\ T = 309^\circ{\cdot}9,\ \theta = 14^\circ{\cdot}72,\ t = 10^\circ{\cdot}99,$

$\therefore c_1 = 0{\cdot}5093.$

Expt. 10.. $W = 74{\cdot}70,\ T = 313^\circ{\cdot}5,\ \theta = 14^\circ{\cdot}61,\ t = 10^\circ{\cdot}83,$

$\therefore c_2 = 0{\cdot}5080,$

and $c^{14\cdot7}_{311\cdot7} = 0{\cdot}5087.$

(ii.) *In Double Calorimeter.*

Weight of platinum cylinder=24·346, W=weight of water and equivalent of calorimeter, &c. I (for platinum), W′=weight of water and equivalent of calorimeter II (for glucinum).

Expt. 11. $W = 95{\cdot}15,\ \theta = 12^\circ{\cdot}52,\ t = 9^\circ{\cdot}53,$

whence $T = 359{\cdot}3^\circ.$

$W' = 93{\cdot}86,\ \theta' = 11^\circ{\cdot}72,\ t' = 10^\circ{\cdot}11,$

and $c^{11\cdot7}_{359\cdot3} = 0{\cdot}5178.$

Expt. 12. $W = 96{\cdot}19,\ \theta = 18^\circ{\cdot}60,\ t = 14^\circ{\cdot}88,$

whence $T = 447^\circ{\cdot}1.$

$W' = 95{\cdot}74,\ \theta' = 17^\circ{\cdot}52,\ t' = 15^\circ{\cdot}50$

and $c^{17\cdot5}_{447\cdot1} = 0{\cdot}5384.$

"On Certain Definite Integrals. No. 13." By W. H. L. Russell, A.B., F.R.S. Received June 18, 1885.

In a paper which will be found in the "Proceedings of the Royal Society" for June, 1865, I gave methods for expressing the sum of certain series by definite integrals, or in other words, of expressing $F(x)$ by the form $\int PQ^x d\theta$. As shown in my last paper, this method is immediately connected with the solution of those partial differential equations which have constant coefficients by definite integrals, a circumstance which never crossed my mind till lately. In the present communication I hope to make further extensions in both these directions.

Case I. It was proved in the paper cited that the function

$$\sqrt[p]{\phi(n)+\sqrt[q]{\chi(n)}}$$

could be expressed in the form $\int PQ^n d\theta$, whereas $\phi(n)$ and $\chi(n)$ are rational (misprinted identical) functions of (n). In the same way we may obtain $\sqrt[p]{\phi(n)+\sqrt[q]{(\chi n+\sqrt[r]{\omega(n)})}}$. For it was proved in that paper that $\sqrt[p]{(\phi n+\sqrt[r]{\chi n})}$ can be expressed in the above form if $\epsilon^{\frac{1}{\chi(n)}}(\chi(n))^{\frac{s}{q}}$ can be thus expressed, and therefore

$$\sqrt[p]{\phi n+\sqrt[q]{(\chi(n)+\sqrt[r]{\omega(n)})}}$$

can be thus expressed in the form $\int PQ^r d\theta$ if

$$\epsilon^{\frac{1}{\chi(n)+\sqrt[r]{\omega n}}}(\chi(n)+\sqrt[r]{\omega(n)})^{\frac{s}{q}}$$

can be expressed in this form, which can be done by repeating the process.

This investigation assumes, however, that $\chi(n)+\sqrt[r]{\omega(n)}$ is less than unity.

Case II. Suppose it were required to reduce ϵ^N, where $N = \sqrt[p]{\phi(n)+\sqrt[q]{\chi(n)+\sqrt[r]{\omega(n)}}}$ to form $\int PQ^n d\theta$.

Then $\epsilon^N=\frac{1}{\pi}\int_0^\pi\frac{\epsilon^{\cos\theta}\cos(\sin\theta)(1-N^2)}{1-2N\cos\theta+N^2}$, and since the denominator can be rationalised, we fall back on Case I. N must of course be less than unity.

Case III. When p is greater than 1

$$F\frac{1}{p}=\frac{p^2-1}{\pi}\int_0^\pi\frac{F\epsilon^{\theta i}+F\epsilon^{-\theta i}}{1-2p\cos\theta+p^2}d\theta,$$

and $p^2-1=p^2-2p\cos\theta+1+2(p-\cos\theta)\cos\theta-2\sin^2\theta$.

Hence

$$\frac{p^2-1}{1-2p\cos\theta+p^2}=1+2\frac{(p-\cos\theta)}{(p-\cos\theta)^2+\sin^2\theta}.\cos\theta$$

$$-\frac{2\sin^2\theta}{(p-\cos\theta)^2+\sin^2\theta}$$

$$=1+2\cos\theta\int_0^\infty \epsilon^{-z(p-\cos\theta)}\cos z\sin\theta d\theta-2\sin\theta\int_0^\infty \epsilon^{-z(p-\cos\theta)}\sin z\sin\theta d\theta.$$

By this means $F\left(\frac{1}{p}\right)$ can be expressed as double integral. So can $F(p)$, but then p must be less than unity.

We will now apply these considerations to the solution of linear partial differential equations.

Let $F\left(\frac{d}{d\xi},\frac{d}{d\eta}\right)u=0$, or as we shall write it, $F\left(x\frac{d}{dx},y\frac{d}{dy}\right)u=0$, then taking as before a specimen term Ax^my^n, m and n must be connected by the relations $F(m,n)=0$. Suppose from this we find

$$m=\sqrt[p]{\phi(n)+\sqrt[q]{\chi(n)+\sqrt[r]{\phi(n)+\quad\ldots}}}$$

Then, as will be seen by the reasoning employed in my former paper, the equation can be solved if

$$\epsilon^{\log_\epsilon x\sqrt[p]{\phi(n)+\sqrt[q]{\chi(n)+\sqrt[r]{\omega(n)+\quad\ldots}}}}$$

can be expressed in the form $\int PQ^n d\theta$, which brings us to Case II.

The same process may in certain cases be applied to partial differential equations with three independent variables. Consider the series $A+Bx+B'y+Cx^2+C'xy+C''y^2+$, . . . when A, B, B' . . . are arbitrary constants. This may be written on Poisson's principles

$$F_1(x)+F_2x.y+F_3(x).y^2+\quad\ldots$$

when F_1, F_2, F_3, . . . are arbitrary functions, and this again $F(x,y)$ when F is an arbitrary function of the two variables.

Now consider the partial differential equation $\frac{du}{d\zeta}=2\frac{d^2u}{d\xi d\eta}$, or as I shall write it $\left(z\frac{d}{dz}\right)u=2\left(x\frac{d}{dx}\right)\left(y\frac{d}{dy}\right)u$, and let $Ax^my^nz^r$ be a specimen term of the solution, as in previous cases, then $r=2mn$, and our object must be to reduce $x^my^nz^{mn}$ to the form $\int PQ_1{}^mQ_2{}^n$; this may be easily done by remembering that $2mn=(m+n)^2-m^2-n^2$, for

$$\int_{-\infty}^{\infty}\epsilon^{-(u-a)^2}du=\sqrt{\pi}$$

Hence $$\int_{-\infty}^{\infty} \epsilon^{2au-u^2}du=\epsilon^{a^2}\sqrt{\pi}$$

and therefore $$\epsilon^{(m+n)^2}=\frac{1}{\sqrt{\pi}}\int_{-\infty}^{\infty}\epsilon^{2(m+n)u-u^2}d\theta$$

also $$\epsilon^{-m^2}=\frac{1}{\sqrt{\pi}}\int_{-\infty}^{\infty}\epsilon^{-\rho^2}\cos 2m\rho d\rho,\text{ and so for }\epsilon^{-n^2}.$$

These transformations give the required form.

If we have two partial differential equations—

$$F_1\left(x\frac{d}{dx},\ y\frac{dx}{dy},\ z\frac{d}{dz}\right)u=0,$$

$$F_2\left(x\frac{d}{dx},\ y\frac{d}{dy},\ z\frac{d}{dz}\right)u=0,$$

then substitute as before $Ax^my^nz^r$ for u; then we have the equations

$$F_1(m, n, r)=0,\ F_2(m, n, r)=0,$$

whence $m=\phi(r)$, $n=\chi(r)$, and we fall back on the first case.

"On Certain Definite Integrals." No. 14. By W. H. L. Russell, A.B., F.R.S. Received June 18, 1885.

It follows from the expansion of $\cos^n\theta$ in terms of the cosines of the multiples of θ, that

$$n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\ \cdots\ \frac{n-r+1}{r}=\frac{2^n}{\pi}\int_0^{\pi}\cos n\theta\cos(n-2r)\theta d\theta,$$

and consequently this theorem can be used in the summation of series involving binomial coefficients. I propose to give a few examples of this.

From the binomial theorem, when the index is even, we have

$$\int_0^{\pi}d\theta\frac{\cos^{2n}\theta\sin(n-1)\theta\cos n\theta}{\sin\theta}=\frac{\pi}{2^{2n}}\left\{2^{2n-1}-1-1-\frac{(2n-1)\ .\ .\ (n+1)}{1\ .\ 2\ .\ .\ .\ (n-1)}\right\}$$

and when the index is odd,

$$\int_0^{\pi}d\theta\frac{\cos^{2n+1}\theta\sin n\theta\cos n\theta}{\sin\theta}=\pi\left\{\tfrac{1}{2}-\frac{1}{2^{2n+1}}\right\}$$

Since $(1+x)^{n-1}=(1+x)^n(1-x+x^2-x^3+\ .\ .\ .\ .\ .\ )$, therefore equating the coefficients of x^r, we have

$$1-n+n\cdot\frac{n-1}{2}-n\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}+\ \ldots(r+1)\text{ terms}$$

$$=(-1)^r\cdot\frac{(n-1)(n-2)(n-3)\ \ldots\ldots\ (n-r)}{1\ .\ 2\ .\ 3\ \ldots\ldots\ r}$$

Hence $$\int_0^\pi \cos^{n-1}\theta d\theta\left\{\cos(n+1)\theta+(-1^r\cos(n-2r+1)\theta)\right\}$$

$$=\frac{\pi}{2^n}(-1)^r\frac{(n-1)(n-2)\ \ldots\ldots\ (n-r)}{1\ .\ 2\ .\ 3\ \ldots\ldots\ r}$$

"The Vortex Ring Theory of Gases. On the Law of the Distribution of Energy among the Molecules." By J. J. THOMSON, M.A., F.R.S., Fellow of Trinity College, Cavendish Professor of Experimental Physics in the University of Cambridge. Received June 4, 1885.

In any kinetic theory of gases the statistical method of investigation must be used, and since the separate molecules of the gas are supposed to possess some properties to very different extents, it is necessary to know how many molecules there are which have the measure of any given property between certain limits. Thus the question of the distribution of configuration and velocity amongst the molecules is one of the most important problems in any theory of gases.

This problem has been solved for the ordinary solid particle theory by Maxwell and Boltzmann, and their researches are the more valuable as the results do not depend on any assumption about the law of force between the molecules.

In this paper I shall attempt to solve the same problem for the vortex atom theory of gases. In this case the question is a little more complicated, as the radii of the vortex rings can vary as well as their velocities. This is one of the most striking differences between the two theories; according to the ordinary theory all the molecules of a gas are of the same size, according to the vortex atom theory the molecules of the same gas vary in size. If this be true, a porous plate of the requisite degree of fineness might play in this theory the part which Maxwell's demons play in the ordinary theory. For let us suppose that we have two chambers, A and B, separated by a porous plate, and that A is filled with gas initially while B is empty, then if the pores in the porous plate are so fine that only the smaller molecules can get through from A to B, then, though some of the molecules will recross the plate, some gas will remain in B, and the

molecules in B will, on the whole, be moving faster than those in A, and so may be supposed to be at a higher temperature, since the smaller the radius of a vortex ring the greater its velocity. Thus B and A might be the hot and cold chambers respectively of a heat engine, and in this way work might be derived from the gas which was originally at a uniform temperature, so that this arrangement would not obey the second law of thermodynamics.

If the molecule on the vortex atom theory of matter consisted of a single ring its velocity of translation would be a function only of its radius. It is, however, for several reasons advisable to take a more general case, and to suppose that the molecule consists of several rings linked through each other, the rings being nearly equal in radius, and also nearly coincident in position; or what is perhaps better, we may suppose that the vortex core forms an endless chain, but that instead of being a single loop like the simple ring, it is looped into a great many coils nearly equal in radius and nearly coincident in position. We may realise this way of arranging the vortex core if we take a cylindrical rod whose length is great compared with its radius, and describe on its surface a screw with n threads so that the threads make m/n turns in the length of the rod, where m is an integer not divisible by n. Then bend the rod into a circle and join the ends, the threads of the screw will form an endless chain with n loops, and we may suppose that this represents the way in which the vortex rings are arranged; it is shewn, however, in my "Treatise on the Motion of Vortex Rings" that this way of arranging the vortex core is unstable if n be greater than six. When the vortex core is arranged in the way just described, the velocity of translation is no longer a function of the size of the ring alone; at the same time when a vortex ring of this kind moves about in a fluid where the velocity is not uniform, the change in the velocity of the ring will be due chiefly to the change in its radius. For the velocity at a small distance d from the circular axis of a vortex ring whose radius is a and strength m is—

$$\frac{m}{2\pi a} \log \frac{8a}{d},$$

so that as $8a/d$ is very large, a change δa in the radius of the ring produces a change in the velocity approximately equal to—

$$-\frac{\delta a}{a} \cdot \frac{m}{2\pi a} \log \frac{8a}{d},$$

while a change δd in the distance of the point from the circular axis of the ring produces a change in the velocity equal to—

$$-\frac{\delta d}{d} \cdot \frac{m}{2\pi a}.$$

Thus for the same relative changes of a and d the changes in the velocities are in the ratio of $\log 8a/d$ to 1, and as $\log 8a/d$ is very great, we may neglect the change in the velocity of the ring produced by the alteration in the distance between the loops in comparison with that produced by the alteration in the size of the ring.

The kinetic energy of a quantity of fluid containing vortex rings of this kind may conveniently be divided into several parts. The first part consists of the kinetic energy of the irrotationally moving fluid surrounding the ring, the second part of the kinetic energy of the rotationally moving fluid; this again may conveniently be divided into two parts, one part being the kinetic energy due to the rotation in the core, and the other that due to the translational velocity of the vortex core.

The kinetic energy of the irrotationally moving liquid surrounding the ring may be expressed in several ways; it is equal to the strength of the ring multiplied by the rate of flow of the fluid through it; the most convenient expression for our purpose, however, is

$$\mathrm{A}va^2,$$

where v is the velocity of translation of the vortex ring resolved along the normal to its plane, a is the radius of the ring and A a constant.

(See p. 12 of my "Treatise on the Motion of Vortex Rings.")

The energy due to the rotation of the vortex core is

$$\tfrac{1}{2}n\pi^2\rho m^2a,$$

where n is the number of loops in the ring, ρ the density of the fluid, and m the strength of the ring.

The kinetic energy due to the translational velocity of the ring is

$$\tfrac{1}{2}\mathrm{M}(u^2+v^2+w^2),$$

where M is the mass of fluid in the ring and $u^2+v^2+w^2$ the square of the velocity of the ring.

Thus if T be the whole kinetic energy due to the ring—

$$\mathrm{T}=\mathrm{A}va^2+\tfrac{1}{2}n\pi^2\rho m^2a+\tfrac{1}{2}\mathrm{M}(u^2+v^2+w^2).$$

Let us consider a vortex ring placed in a fluid where there is a velocity potential Ω independent of that due to the vortex ring itself, the value of Ω is supposed to be known at every point of the fluid.

We have to fix the position, size, and motion of the ring. We can do this if we know the coordinates (x, y, z) of its centre, its radius (a), the direction cosines (l, m, n) of its plane, and V that part of the velocity at the ring which is due to the ring itself. V is not necessarily the actual velocity of the ring, for this latter quantity is the

resultant of V, and the velocity whose components are $d\Omega/dx$, $d\Omega/dy$, $d\Omega/dz$.

Let $\xi=la^p$, $\eta=ma^p$, $\zeta=na^p$, $\omega=V^q$.

Then we shall prove that it is possible to determine p and q so that the number of molecules which have the values of $x, y, z, \xi, \eta, \zeta, \omega$, between $x, y, z, \xi, \eta, \zeta, \omega$, and $x+dx$, $y+dy$, $z+dz$, $\xi+d\xi$, $\eta+d\eta$, $\zeta+d\zeta$, $\omega+d\omega$, and for which the kinetic energy of the molecule and the surrounding fluid is T, is when the gas is in a uniform and steady state—

$$Ce^{-h\text{T}}dxdydzd\xi d\eta d\zeta d\omega,$$

where C is some constant determined by the number of molecules in the gas.

We shall first prove that this represents a possible distribution among the molecules of the quantities denoted by ξ, η, ζ, ω, when the vortex rings are moving in a fluid whose velocity varies from point to point; we disregard for the present the effects of any collisions which may take place among the vortex rings themselves. In this case the rings are supposed to be so far apart that they do not influence each other, so that the velocity of any ring is the same as if the others did not exist. T represents the kinetic energy due to the ring and the distribution of velocity potential Ω on this supposition.

We have to prove that if the distribution be represented by this expression at any time, it will continue to be represented by it. This will be the case if the expression

$$Ce^{-h\text{T}}dxdydzd\xi d\eta d\zeta d\omega,$$

remains constant as the molecules move about. Now T, the kinetic energy, remains constant, so that we have to prove that

$$dxdydzd\xi d\eta d\zeta d\omega$$

also remains constant.

Since Ω is the part of the velocity potential which is not due to the rings themselves, by the equations on pages 65 and 66 of my "Treatise on the Motion of Vortex Rings," we have—

$$\frac{da}{dt}=-\tfrac{1}{2}\,a\frac{d^2\Omega}{dh^2},$$

$$\frac{dl}{dt}=l\frac{d^2\Omega}{dh^2}-\frac{d^2\Omega}{dhdx},$$

$$\frac{dm}{dt}=m\frac{d^2\Omega}{dh^2}-\frac{d^2\Omega}{dhdy},$$

$$\frac{dn}{dt}=n\frac{d^2\Omega}{dh^2}=\frac{d^2\Omega}{dhdz};$$

where
$$\frac{d}{dh}=l\frac{d}{dx}+m\frac{d}{dy}+n\frac{d}{dz}.$$

so that if δa, δl, δm, δn be the changes in l, m, n respectively in the small time τ then—

$$\delta a=-\tfrac{1}{2}a\frac{d^2\Omega}{dh^2}\tau,$$

$$\overline{\delta l}=\left(l\frac{d^2\Omega}{dh^2}-\frac{d^2\Omega}{dhdx}\right)\tau,$$

$$\delta m=\left(m\frac{d^2\Omega}{dh^2}-\frac{d^2\Omega}{dhdy}\right)\tau,$$

$$\delta n=\left(n\frac{d^2\Omega}{dh^2}-\frac{d^2\Omega}{dhdz}\right)\tau,$$

and if δx, δy, δz are the changes in x, y, z in the time τ,

$$\delta x=u\tau,$$

$$\delta y=v\tau,$$

$$\delta z=w\tau,$$

where u, v, w are the component velocities of the centre of the vortex ring.

Let x', y', z', ω', ξ', η', ζ' be the values of x, y, z, ω, ξ, η, ζ respectively after the time τ, then—

$$x'=x+u\tau,$$

$$y'=y+v\tau,$$

$$z'=z+w\tau,$$

$$\omega'=\omega+\delta\omega,$$

but since
$$\omega=V^q,$$

$$\delta\omega=qV^{q-1}\delta V.$$

Now the change in V will be due to the change in the shape and size of the ring, and as we saw before that this is due almost entirely to the change in the radius, thus the change in V will be $-V\delta a/a$, or substituting for δa its value—

$$\delta V=\tfrac{1}{2}V\frac{d^2\Omega}{dh^2}\tau,$$

thus
$$\omega'=\omega+\tfrac{1}{2}q\omega\frac{d^2\Omega}{dh^2}\tau,$$

$$\xi' = \xi + \delta\xi$$
$$= \xi + pa^{p-1}l\delta a + a^p\delta l.$$

Substituting for δa and δl, their values, we get

$$\xi' = \xi - \tfrac{1}{2}p\xi\frac{d^2\Omega}{dh^2}\tau + \xi\frac{d^2\Omega}{dh^2}\tau - \left(\xi\frac{d^2\Omega}{dx^2} + \eta\frac{d^2\Omega}{dxdy} + \zeta\frac{d^2\Omega}{dxdz}\right)\tau.$$

Similarly,

$$\eta' = \eta - \tfrac{1}{2}p\eta\frac{d^2\Omega}{dh^2}\tau + \eta\frac{d^2\Omega}{dh^2}\tau - \left(\xi\frac{d^2\Omega}{dxdy} + \eta\frac{d^2\Omega}{dy^2} + \zeta\frac{d^2\Omega}{dxdz}\right)\tau,$$

$$\zeta' = \zeta - \tfrac{1}{2}p\zeta\frac{d^2\Omega}{dh^2}\tau + \zeta\frac{d^2\Omega}{dh^2}\tau - \left(\xi\frac{d^2\Omega}{dxdz} + \eta\frac{d^2\Omega}{dydz} + \zeta\frac{d^2\Omega}{dz^2}\right)\tau.$$

If we neglect the squares of δx, δy, δz, δw, $\delta\xi$, $\delta\eta$, $\delta\zeta$,

$$dx'dy'dz'dw'd\xi'd\eta'd\zeta' = dxdydzdwd\xi d\eta d\zeta\left\{1 + \left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)\tau\right.$$
$$\left. + \frac{d\delta w}{dw} + \frac{d\delta\zeta}{d\zeta} + \frac{d\delta\eta}{d\eta} + \frac{d\delta\zeta}{d\zeta}\right\}.$$

Now
$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

And by the equations written above

$$\frac{d.\delta w}{dw} = \tfrac{1}{2}q\frac{d^2\Omega}{dh^2}\tau,$$

and
$$\frac{d.\delta\xi}{d\xi} + \frac{d.\delta\eta}{d\eta} + \frac{d.\delta\zeta}{d\zeta} =$$
$$\tau\left\{3(1-\tfrac{1}{2}p)\frac{d^2\Omega}{dh^2} + (1-\tfrac{1}{2}p)\left(\xi\frac{d}{d\xi} + \eta\frac{d}{d\eta} + \zeta\frac{d}{d\zeta}\right)\frac{d^2\Omega}{dh^2}\right.$$
$$\left. - \left\{\frac{d^2\Omega}{dx^2} + \frac{d^2\Omega}{dy^2} + \frac{d^2\Omega}{dz^2}\right\}\right\}.$$

But
$$\frac{d^2\Omega}{dx^2} + \frac{d^2\Omega}{dy^2} + \frac{d^2\Omega}{dz^2} = 0,$$

and since

$$\frac{d^2\Omega}{dh^2} = \left\{\frac{\xi}{\{\xi^2+\eta^2+\zeta^2\}^{\frac{1}{2}}}\frac{d}{dx} + \frac{\eta}{\{\xi^2+\eta^2+\zeta^2\}^{\frac{1}{2}}}\frac{d}{dy} + \frac{\zeta}{\{\xi^2+\eta^2+\zeta^2\}^{\frac{1}{2}}}\frac{d}{dz}\right\}^2\Omega,$$

we see that $\dfrac{d^2\Omega}{dh^2}$ is a homogeneous function of ξ, η, ζ of zero dimensions, and therefore by Euler's theorem

$$\left\{\xi\frac{d}{d\xi} + \eta\frac{d}{d\eta} + \zeta\frac{d}{d\zeta}\right\}\frac{d^2\Omega}{dh^2} = 0,$$

hence
$$dx'dy'dz'dw'd\xi'd\eta'd\zeta'$$
$$=dxdydzdwd\xi d\eta d\zeta\left(1+\{\tfrac{1}{2}q+3(1-\tfrac{1}{2}p)\}\frac{d^2\Omega}{dh^2}\right),$$

so that if
$$q=3(p-2)$$
$$dx'dy'dz'dw'd\xi'd\eta'd\zeta'=dxdydzdwd\xi d\eta d\zeta,$$

and therefore the distribution represented by the expression
$$Ce^{-h\mathrm{T}}dxdydzdwd\xi d\eta d\zeta,$$

will be permanent if there are no collisions between the molecules.

We shall now go on to shew that this expression will represent the distribution of coordinates and momenta among the molecules even when collisions take place, at any rate if the collisions are not very violent.

Let us call the group of molecules which have the quantities $x, y, z, \xi, \eta, \zeta, w$ between $x, y, z, \xi, \eta, \zeta, w$, and $x+dx$, $y+dy$, $z+dz$, $\xi+d\xi$, $\eta+d\eta$, $\zeta+d\zeta$, $w+dw$ the group A. The number of molecules in this group is
$$Ce^{-h\mathrm{T}}dxdydzdwd\xi d\eta d\zeta,$$

all the symbols having the same meaning as before.

Let us consider another group of molecules which have their coordinates between $x_1, y_1, z_1, \xi_1, \eta_1, \zeta_1, w_1$, and x_1+dx_1, y_1+dy_1, z_1+dz_1, $\xi_1+d\xi_1$, $\eta_1+d\eta_1$, $\zeta_1+d\zeta_1$, w_1+dw_1. We shall call this group B. The number of molecules in this group is
$$De^{-h\mathrm{T}_1}dx_1dy_1dz_1d\xi_1,\ d\eta_1d\zeta_1dw_1.$$

We shall suppose that the molecules of the A group come into collision with those of the B group, and that the values of the co-ordinates after the collision are denoted by putting dashes to the letters which denoted the corresponding coordinates before the collision.

In my "Treatise on the Motion of Vortex Rings" it is proved that the effects of a collision depend on, in addition to the quantities already specified, the angle which the line joining the centres of the rings when they are nearest together makes with the shortest distance between the directions of motion of the rings; let us call this angle ϕ. ϕ is positive for the ring which first passes through the shortest distance between the directions of motion of the ring, negative for the other ring, and it may have any value between $-\pi/2$ and $\pi/2$.

We may suppose that a collision takes place when the shortest distance between the centres of the two rings is less than some assigned value; it is not, however, necessary to limit ourselves to any particular way of defining a collision.

Let $\gamma d\phi$ be the fraction of the number of pairs of molecules which come into collision in the unit of time and contain one molecule from the group A and another from the group B, and for which ϕ is between ϕ and $\phi+d\phi$. Then if the states in which the A and B molecules are in after the collision be called A′ and B′ respectively, the number of pairs of molecules which in the unit of time leave the state {AB} and enter the state {A′B′} is—

$$\mathrm{CD}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\phi\{e^{-h(\mathrm{T}+\mathrm{T}_1)}\gamma dxdydzd\xi d\eta d\zeta d\omega dx_1dy_1dz_1d\xi_1d\eta_1d\zeta_1d\omega_1\}.$$

Now the distribution will be steady if this equals the number of molecules which leave the state {A′B′} in the unit of time, but this number is—

$$\mathrm{CD}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\phi\{e^{-h(\mathrm{T}'+\mathrm{T}'_1)}\gamma' dx'dy'dz'd\xi'd\eta'd\zeta'd\omega'dx'_1dy'_1dz'_1d\xi'_1d\eta'_1d\zeta'_1d\omega'_1\}.$$

We can see that if the gas does not exhibit vector properties γ must equal γ'. For since the motion is reversible, if any two molecules whose coordinates are ξ, η, ζ, ω; ξ_1, η_1, ζ_1, ω_1, come into collision, then the molecules whose coordinates are $-\xi$, $-\eta$, $-\zeta$, ω; $-\xi_1$, $-\eta_1$, $-\zeta_1$, ω_1, will also collide. Let these latter molecules be said to be in the states (−A′), (−B′) respectively. Thus the percentage of collision for the states (AB), that is for collisions between two molecules in the states A and B, respectively is the same as for the state (−A′, −B′). But since as many molecules are moving in any direction as in the opposite, the number of molecules in the state −A′ will equal the number in the state A′, and similarly the number of molecules in the state −B′ is the same as the number in the state B′, and since the gas exhibits no vector properties, the mean path between the collisions between the molecules in the states A′ and B′ must equal the mean path between the collisions between the molecules in the states −A′ and −B′; and thus the percentage of collisions must be the same. So that the percentage for the state (A′B′) equals the percentage for the state (—A′, −B′), but this, as we saw, equals the percentage for the state AB; and, therefore, the percentage for the state AB equals the percentage for the state (A′B′); or $\gamma=\gamma'$. Since the collision may be fixed with regard to either molecule, and since ϕ is positive for one molecule, negative for the other, we see that γ cannot change sign with ϕ, so that if γ is a function of ϕ it must be one of the form—

$$\gamma=\gamma_0+\gamma_1\cos\phi+\gamma_2\cos 2\phi+\ldots.$$

Again,
$$dxdydz = dx'dy'dz'$$
$$dx_1dy_1dz_1 = dx'_1dy'_1dz'_1$$

and none of the quantities are functions of ϕ.

We have also since the total kinetic energy is not changed by the collision $T+T_1=T'+T_1'$, and neither of these quantities is a function of ϕ. Since this is so, we see that the expressions we have assumed will represent a steady distribution if—

$$\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \gamma d\phi \{d\xi d\eta d\zeta dw d\xi_1 d\eta_1 d\zeta_1 dw_1\} = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \gamma d\phi (d\xi' d\eta' d\zeta' dw' d\xi'_1 d\eta'_1 d\zeta'_1 dw'_1).$$

Let us suppose that the collisions are not violent enough to make the vortex rings deviate greatly from their circular forms, and let us consider the effect produced on an A molecule by collision with a B molecule. Let Ω' be the potential due to the B molecule, then just as before we have—

$$w' = w + \tfrac{1}{2}qw\int_{-\infty}^{+\infty} \frac{d^2\Omega}{dh^2}dt$$

$$\xi' = \xi + (1-\tfrac{1}{2}p)\xi\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dh^2}dt - \left\{\xi\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dx^2}dt + \eta\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dxdy}dt + \zeta\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dxdz}dt\right\},$$

with similar expressions for η' and ζ'. Here h is drawn along the normal to the A molecule, and the coordinates are supposed to be changed by the collision by only a small fraction of their values.

Now the only thing that makes any difference between this case and the former one is that now $\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dh^2}dt$ is a function of v, and therefore of w. If therefore we assume that $3(p-2)=q$, we have

$$d\xi' d\eta' d\zeta' dw' = d\xi d\eta d\zeta dw\left\{1 + \tfrac{1}{2}qw\frac{d}{dw}\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dh^2}dt\right\}$$

Now $\int_{-\infty}^{+\infty}\frac{d^2\Omega}{dh^2}dt$ is proportional to the change in w, and therefore, by § 29 of my "Treatise on Vortex Motion" is of the form $f \sin 3\phi$, where f is a function of w but not of ϕ, thus :—

$$d\xi' d\eta' d\zeta' dw' = d\xi d\eta d\zeta dw\{1 + \tfrac{1}{2}qw\frac{df}{dw}\sin 3\phi\}.$$

Similarly

$$d\xi'_1 d\eta'_1 d\zeta'_1 dw'_1 = d\xi_1 d\eta_1 d\zeta_1 dw_1\{1 + \tfrac{1}{2}qw_1\frac{df}{dw_1}\sin 3\phi\},$$

so that neglecting the squares of small quantities

$$d\xi' d\eta' d\zeta' dw' d\xi'_1 d\eta'_1 dw'_1 = d\xi d\eta d\zeta dw d\xi_1 d\eta_1 d\zeta_1 dw_1 \{1 + \tfrac{1}{2} qw \frac{df}{dw} \sin 3\phi + \tfrac{1}{2} qw' \frac{df'}{dw'} \sin 3\phi\},$$

and therefore

$$\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \gamma d\phi \{d\xi' d\eta' d\zeta' dw'_1 d\xi'_1 d\eta'_1 d\zeta' dw\} = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \gamma d\phi \{d\xi d\eta d\zeta dw d\xi_1 d\eta_1 d\zeta_1 dw_1\}.$$

Since we see from the form of γ that—

$$\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \sin 3\phi \gamma d\phi = 0.$$

Thus the condition for a steady distribution is satisfied, and we therefore conclude that a possible distribution of the values of the coordinates among the molecules of the gas is represented by the expression—

$$Ce^{-h\text{T}} d\xi d\eta d\zeta dw dx dy dz,$$

where $\xi = la^p, \ \eta = ma^p, \ \zeta = na^p, \ w = v^q$

and $3(p-2) = q.$

Let us consider the case when there is no external disturbance in the fluid containing the vortex rings; the distribution will be uniform in all parts of the fluid, so that the number of molecules which have the quantities ξ, η, ζ, w, between ξ, η, ζ, w and $\xi + d\xi$, $\eta + d\eta$, $\zeta + d\zeta$, $w + dw$ is independent of x, y, z, and so by the above formula will be proportional to—

$$e^{-h\text{T}} d\xi d\eta d\zeta dw,$$

or if the normals to the planes of the vortex rings point uniformly in all directions the number of molecules which have a between a and $a + da$, v between v and $v + dv$ is proportional to—

$$e^{-h\text{T}} a^{3p-1} v^{q-1} da dv,$$

or substituting for q the value $3(p-2)$

$$e^{-h\text{T}} a^{3p-1} v^{3p-7} da dv.$$

Though in the kind of molecule we are considering, a and v may be treated as independent variables, still the limits of v depend upon the value of a. For suppose the molecule to consist of n rings

linked together, then for a given value of a the velocity of the molecule will be least when the links are so far apart that they do not greatly affect each other's velocity; in this case v will equal—

$$\frac{m}{2\pi a}\log\frac{8a}{e},$$

the velocity of the molecule will be greatest when the n rings are close together; in this case v will equal—

$$\frac{nm}{2\pi a}\log\frac{8a}{e}.$$

So that if we integrate first with respect to v we must do so between these limits.

Since, however—

$$T = Aa^2v + \tfrac{1}{2}nm^2\pi^2\rho a + \tfrac{1}{2}Mv^2,$$

we cannot perform the integration except between the limits zero and infinity for both a and v; if, however, n be large, or the molecule complicated, the results got by integration between the limits—

$$\frac{m}{2\pi a}\log\frac{8a}{e} \text{ and } \frac{mn}{2\pi a}\log\frac{8a}{e}$$

for v and zero and infinity for a will not differ much from those got by integrating between zero and infinity for both a and v.

The second term in the expression for the kinetic energy is very small compared with the first, so that it may be neglected without causing sensible error. We shall find it convenient to take as new variables the two remaining terms in the expression for the kinetic energy; we shall call these new variables α and β respectively, where α denotes the energy in the fluid surrounding the ring, β the energy due to the translational velocity of the ring, so that—

$$Aa^2v = \alpha$$

$$\tfrac{1}{2}Mv^2 = \beta,$$

and therefore

$$da\,dv = \frac{1}{MAav^2}d\alpha\, d\beta,$$

so that

$$Ce^{-kT}\alpha^{3p-1}v^{3p-7}da\,dv = C'e^{-k(\alpha+\beta)}\alpha^{\frac{3p-2}{2}}\beta^{\frac{3p-16}{4}}d\alpha\, d\beta,$$

where C' is a new constant.

Thus the number of molecules which have the energy in the fluid surrounding them between α and $\alpha+\delta\alpha$, and also the energy due to the translational velocity of the ring between β and $\beta+\delta\beta$ is—

$$C' e^{-h(\alpha+\beta)} \alpha^{\frac{3p-2}{2}} \beta^{\frac{3p-10}{4}} d\alpha d\beta,$$

and if the molecule is so complex that α and β may be regarded as independent, then the limits of α and β are zero and infinity.

The quantity p is at present undetermined.

Let us apply this result to find the pressure of a gas on the sides of the vessel which contains it. To do this we must consider what takes place at the sides of the vessel. The general nature of this action was described by Sir William Thomson ("Nature," vol. xxiv, p. 47). As the vortex rings move up to the sides of the vessel they swell out and move slowly up the bounding surface, where they form a layer of swollen vortices sticking to the sides of the vessel. A vortex ring coming up to the surface tends to wash off the vortex rings attached to the surface on either side of it, so that when things have got into a state of equilibrium there is a vortex ring washed off for each one that comes up. Thus the pressure on the surface of the vessel will be the same as if the vortex ring struck against the surface and was reflected away again with its velocity reversed, if we assume, as seems natural, that the average velocity of the rings leaving the surface is the same as of those approaching it. Thus each ring that comes up may be looked upon as communicating twice its momentum to the surface, and we can explain the pressure of a gas, just as in the ordinary theory. We have to remark here, however, that the phrase momentum of the vortex ring is ambiguous, as there are two different momenta connected with the ring; there is (1) the momentum of the ring and the fluid surrounding it; and (2) the momentum of the fluid forming the ring alone; this is proportional to the velocity of the ring, while (1) is not only not proportional to the velocity, but in the single ring decreases as the velocity of the ring increases; in a very complex ring it does not necessarily do this, but even in this case it is not proportional to the velocity.

Now, when a vortex ring gets stopped by a surface the question arises whether the momentum communicated to the surface is the momentum (1) or (2). The answer to this question depends on what we consider the nature of the surface to be. If the surface stops the fluid as well as the ring, then no doubt (1) is the momentum which is communicated to the surface. If, however, the surface stops the ring but allows the greater part of the fluid to flow on, then the momentum communicated to the surface is evidently approximately equal to (2). If we consider that the surface is formed of vortex rings the latter supposition seems the more probable, as the fluid in which the rings move can hardly be supposed to be stopped by such a porous surface. We may illustrate this by a mechanical analogy. Let us suppose that we have a number of anchor rings with circulation

established round them moving about in water, and striking against a grating immersed in it. The momentum of the anchor ring will consist of two parts, one due to the circulation, the other due to the translational velocity of the ring. If the grating is so fine that the openings are only a small fraction of the whole area, then the momentum communicated to the grating will be the whole momentum; if, however, the grating is a coarse one, so that the openings form the larger portion of the area, then the momentum communicated to the grating will only be the momentum of the ring itself. And this seems to correspond to the case of vortex motion.

Thus if a be the velocity of the gas resolved along the normal to the boundary surface, the pressure on the surface per unit of area or the momentum communicated to it per unit of time is—

$$\begin{aligned} &2\Sigma \mathrm{M}a^2 \\ &= \tfrac{2}{3}\Sigma \mathrm{M}v^2 \\ &= \tfrac{4}{3}\Sigma\beta, \end{aligned}$$

using the same notation as before.

Now, the number of molecules which have the quantities α and β between α, β and $\alpha+d\alpha$, $\beta+d\beta$ is proportional to—

$$e^{-h(\alpha+\beta)}\alpha^{\frac{3p-2}{2}}\beta^{\frac{3p-16}{4}}d\alpha d\beta,$$

so that if N be the number of molecules—

$$\frac{\Sigma\beta}{\mathrm{N}} = \frac{\Gamma\left(\frac{(3p-8}{4}\right)}{h\Gamma\left(\frac{3p-12}{4}\right)} = \frac{1}{h}\frac{3p-12}{4}$$

where $\Gamma(n)$ is written for

$$\int_0^\infty e^{-x}x^{n-1}dx$$

and the molecule is supposed to be so complex that we may, without sensible error, suppose the limits of α and β to be zero and infinity.

We may take $1/h$ as proportional to the temperature θ of the gas, since it is the same for each of two gases which are in contact with each other, and is also proportional to the mean kinetic energy of the rings themselves.

Substituting the above value for $\epsilon\beta$, we see that the pressure equals

$$(p-4)\frac{\mathrm{N}}{h},$$

and thus varies as $\mathrm{N}\theta$.

Thus Boyle and Gay-Lussac's laws follow from the vortex atom theory.

At present the quantity p is quite undetermined. It could be determined by comparing the coefficients of viscosity and the conductivity for heat of the gas, since these depend on the mean values of different powers of β, and the ratio of such quantities evidently depends on p. In this paper I shall not consider the theory of the conduction of heat. If it were the same on the vortex atom theory of gases as on the ordinary theory, then the distribution of velocities would follow Maxwell's law, as the values of the ratio of the coefficient of viscosity to the conductivity deduced from this law agree fairly well with experiment. For this to be the case—

$$\frac{3p-16}{4}=\frac{1}{2},$$

or

$$p=6,$$

and the number of molecules which have the quantities α and β between α and $\alpha+\delta\alpha$, β and $\beta+\delta\beta$ would be proportional to—

$$e^{-h(\alpha+\beta)}\alpha^{8}\beta^{\frac{1}{2}}d\alpha d\beta.$$

"The History of the Kew Observatory." By ROBERT HENRY SCOTT, M.A., F.R.S., Secretary to the Meteorological Council. Received and read June 18, 1885.*

THE building, known by a misnomer of at least half a century's date as the Kew Observatory, while it is really situated at Richmond, is erected on, or close to, a part of the foundations of a much earlier structure, the old Carthusian Priory of Jesus of Bethlehem.

We learn from Crisp's "Richmond" that "the ancient hamlet of West Sheen occupied that portion of land now known as the Richmond Gardens, or Old Deer Park, and for the site of which hamlet or village we may perhaps take with tolerable correctness the present Observatory as the centre."

The Observatory is situated upon a low mound, which is apparently artificial. The central part of the building stands upon vaulting constructed of bricks, differing in character from modern "stock" bricks, being soft, red, thinner and narrower. Similar bricks are to be found in the walls of Richmond Palace (Crisp, p. 123), and such have been mainly used in the construction of the basement of the Observatory, up to the stone course.

The basement is surrounded by three successive square rings of vaulting, of which the innermost is 5 feet wide by 8 feet high, the second 8 feet by 6 feet high, and the third and last 6 feet 6 inches by 5 feet high. This vaulting is constructed of bricks similar to those used in the upper part of the building, which resemble the bricks of the present day.

Crisp's statement, given above, is not absolutely exact, for the topographical history of the plot of land bounded on the one side by the bend of the river, and on the other by the present high road from Richmond to Kew, the old "Kew Lane" (see Fig. 3, p. 46), is rather complicated. Three separate domains can, however, be recognised—

1. Kew Gardens.
2. Richmond Gardens.
3. The Old Deer Park.

Of the three domains Nos. 1 and 2 were separated by a bridle path called "Love Lane," which started from West Sheen Lane near Richmond Green, and ran in a north and south direction to the Horse

* I am indebted to the kindness of several friends for much assistance in the preparation of this history, particularly to Dr. E. W. Bond, of the British Museum, Mr. W. Thiselton Dyer, F.R.S., and to the members of the staff of the Observatory. —R. H. S.

Ferry at Brentford. Nos. 2 and 3 were quite distinct, though contiguous to each other, whereas the above quotation from Crisp would convey the idea that the two names, Richmond Gardens and the Old Deer Park, were applied indiscriminately to the same area.

1. Kew Gardens lay to the east of Love Lane, they were the gardens of Kew House, of which Frederick Prince of Wales (son of George II) took a long lease from Mr. S. Molyneux, his secretary, to whom it had passed by his marriage with Lady Elizabeth, grand-niece of Lord Capel.

Mr. Samuel Molyneux, F.R.S., had erected an observatory in a wing of the house, in which he in the year 1725 made, with a telescope of his own construction, in conjunction with Bradley, the famous observations which, after his death, were continued by Bradley and proved the Aberration of Light. *This was the original and real Kew Observatory.*

Kew House was taken down in 1803, and the present sundial on its site erected by William IV, in 1832. The inscription on that dial* hardly gives sufficient credit to Molyneux, to whom, however, Bradley does full justice in Phil. Trans., Vol. XXXV, No. 406, p. 637.

2. Richmond Gardens were the gardens of Richmond Lodge, formerly Ormonde House; of this area 37 acres, including 12 taken from the Old Deer Park, are still in the occupation of the Royal Family.

As Dr. Evans, in his "Richmond and its Vicinity" (2nd Ed., 1825), says (p. 12), "Richmond Gardens existed and were in the zenith of their popularity before Kew Gardens emerged into distinction."

3. The Old Deer Park was the park of the same house, which stood between it and the gardens.

As to the origin of Richmond Lodge, we have to go further back. Richmond Palace (or the Palace of Sheen, as it was called before Henry VII gave the village the name of Richmond) was the Sheen Manor House. It was situated on the south-west side of Richmond Green, near the river, and of it little remains save an archway with the Tudor Arms and parts of the outer walls. Edward I made it a palace, and it continued so until the time of Charles I. Edward III, Henry VII, and Elizabeth all died there. Under the Commonwealth in 1650 it was sold, and after the Restoration was again in the hands of the Crown, and it had been mostly pulled down in the seventeenth

* The inscription upon the dial is as follows :—"On this spot, in 1725, the Rev. James Bradley made the first observations which led to his two great discoveries—the Aberration of Light and the Nutation of the Earth's Axis. The telescope which he used had been erected by Samuel Molyneux, Esq., in a house which afterwards became a Royal residence, and was taken down in 1803. To perpetuate the memory of so important a station, this dial was placed on it in 1832, by command of His Most Gracious Majesty King William the Fourth."

century. On a part of the site the Duke of Queensberry's house was built.

Richmond Lodge, which in its turn became the Palace of Richmond, was apparently originally the Lodge of the Palace Park, the Old Deer Park. It was situated near the present Observatory. The gardens were in front of it, the park at the back.

As regards the position of the Priory, I have learnt from Dr. Bond, of the British Museum, that all the recognised authorities agree in placing the Carthusian Monastery to the north or the north-west of the old Palace of Sheen. Crisp gives an engraving of the Monastery before its demolition, and in an engraving of a panoramic view of Richmond by Anthony van de Wyngaerde, dated 1562, a building resembling Crisp's view is shown in the site of the Priory indicated in the subjoined map.

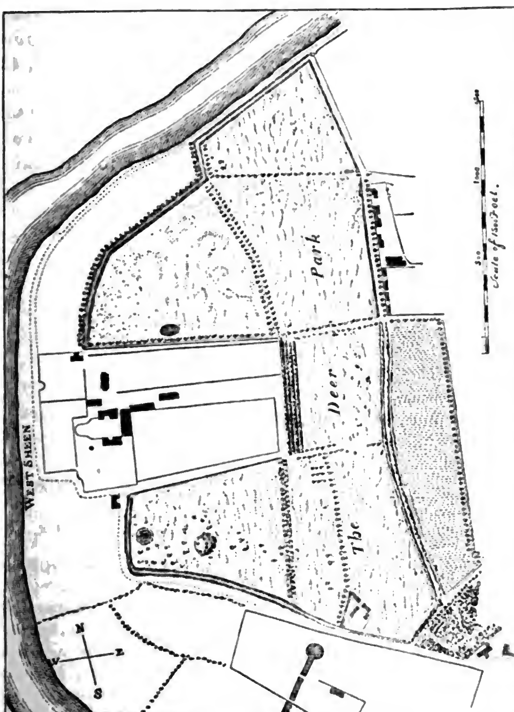

FIG. 1.—SITE OF THE CARTHUSIAN PRIORY, WEST SHEEN, CIRCÂ 1730.

From a Portfolio marked K, 46, 16 h, in the King's Library, British Museum.

We now come to the original buildings on the actual site of the Observatory, and we find in Crisp the following statement:—

"It was in the year 1414 that Henry V, to expiate, as it has been said, the crime by which his family had attained to the crown of England, namely, the dethronement and murder of the unfortunate Richard II, founded here a 'famous' religious house for forty monks of the Carthusian order, under the name of 'The House of Jesus of Bethlehem at Sheen,' by which name it was incorporated, and one John Wydrington constituted the first prior.

"At the same time that Henry founded this noble priory, he likewise built and endowed another one at 'Syon' on the opposite bank of the river, where the present Syon House now stands; this he dedicated to St. Bridget, for sixty nuns of that order."

Crisp says, "There is in the British Museum an old work, in which mention is made of these two 'relygious houses,' and that 'it is there stated they were founded for the reason that a constant succession of holy exercises should be kept up night and day to the end of time, so that when the devotions at one convent had been concluded, at the other they should instantly begin.

"Shakespeare had learned from the works of old chroniclers and historians the reasons given for the institution of these two (houses), as in his 'Henry V' he makes the king, prior to the battle of Agincourt, utter the following words:—

"'Not to-day, O Lord,
O, not to-day think thou upon the fault
My father made in compassing the crown!
I Richard's body have interred anew,
And on it have bestowed more contrite tears
Than from it issued forced drops of blood.
Five hundred poor I have in yearly pay,
Who twice a day their withered hands hold up
Toward Heaven, to pardon blood; and I have built
Two chantries, where the sad and solemn priests
Still sing for Richard's soul.'

"These buildings of Sheen and Syon were both of them stately edifices, and were as nobly endowed."

In 1541 the monastery, along with others, was suppressed.*

* Extract from Archæologia Soc. Antiq., Lond., vol. xx, App. pp. 575, 576:—

"June 8, 1820. William Bray, Esq., Treasurer, exhibited to the Society an impression from the seal of the Carthusian Priory, which formerly existed at Shene, near Richmond, in Surrey; appended to an indenture between John Bokyngham, prior of that house, on the one hand, and John and Joan Rede, of Lewisham, in Kent, on the other, respecting a garden or toft in East Greenwich, dated in the 22nd year of Henry the Sixth.

"The impression of the seal is small, of an oval shape, and has a representation of the Adoration of the Shepherds in the area. At bottom are the arms of France and

Crisp says further: "It was in the year 1770* that the village or hamlet of West Sheen with the ancient gateway forming the entrance to, or rather part of, the priory, and eighteen houses with large pieces of ground attached, were pulled down, and the entire site converted into park or pasture land, as we now see it; but the antiquary to whom the records of such institutions as this 'House of Jesus of Bethlehem' are so dear, while pondering over the changes which have taken place in Richmond, and observing how little we now retain of so much which has once existed here as the work of our Norman, Plantagenet, and Tudor kings, can but cherish a feeling of the deepest regret at the total annihilation of the ancient priory buildings of Henry V at Sheen."

Richmond Lodge, or House (once occupied by Cardinal Wolsey), which stood at no great distance from the present Observatory, had been granted in 1707 by Queen Anne to the Duke of Ormonde, and partly rebuilt by him, in the year 1708–9, on the site of an old building which had likewise borne the name of the Lodge for a long period of years. On the impeachment of the duke in 1715, he hastily left the country, and resided at Paris. Ormonde House was apparently unfinished at the time. The Earl of Arran, his brother, who purchased the property, then leased for the term of about ninety years, sold the lease to the Prince of Wales, afterwards George II, of whom, both before and after his succession to the throne, it was a favourite place of residence, and even more particularly so of his queen (Caroline). From this cause a numerous circle gathered in and about the village and neighbourhood of Richmond, forming here the court of the reigning monarch.

Here, in the garden appertaining to this lodge, took place the interview between Queen Caroline and Jeannie Deans, after her journey on foot from Edinburgh to plead for the life of her sister Effie, which has been so graphically and so touchingly described by Sir Walter Scott in his "Heart of Midlothian."

There is one passage in the dialogue which has a connection with the site of the Observatory, and that is Jeannie's reply to the Queen when addressed in the following words:—

"Stand up, young woman, and tell me what sort of a barbarous people your countryfolk are, where child murder is become so common as to require the restraint of laws like yours."

"If your Leddyship pleases, there are many places besides Scotland where mithers are unkind to their ain flesh and blood."

For as Crisp says, "It cannot be denied that the behaviour of Caroline had been unnatural towards her son; she seems to have

England quarterly. The inscription round, when read at length, is—*Sigillum Domus Jhesu Christi de Bethlem Ordinis Cartusiensis de Shene.*"

* 1769, Evans.

hated him thoroughly and intensely—slighted his young and amiable wife—sided with his father, who upon all occasions behaved towards him with harshness and severity; and when on her deathbed the prince importuned to be allowed to see her, and sent her a most affectionate message, refused to have him admitted to her presence." And therefore "of this famous Richmond Lodge, its magnificent gardens, the statuary and the numerous and singular buildings with which the Queen of George II had at such an extraordinary outlay enriched the place, the remains of the ancient monastery of Sheen, the large and embattled Gothic entrance, and the numerous houses still appertaining to the hamlet—we have now not a vestige left." For "a few years after the accession of George III, the public, more especially of Richmond and Kew, were surprised to learn that it was His Majesty's intention to pull down the whole of the buildings and convert the estate into a large pasturage for cattle, which intention was duly carried out."

"It was at the time asserted, and in that assertion there is no doubt much truth, that the young King so detested the memory of his grandmother, Queen Caroline, so cherished a recollection of the unnatural behaviour which she had always shown towards his late father, Frederick, Prince of Wales, that he took an earnest pleasure in destroying all that she had erected, or on which her taste and resources had been expended."

To return to the topographical description of the property. The Brentford Horse Ferry was superseded by the erection of the first Kew Bridge in 1759.

In 1765 George III obtained an Act (6 George III) for the shutting up of Love Lane, undertaking in return to maintain Kew Lane, the present high road from Richmond to Kew Bridge. This Act was apparently ineffective, for a further one was passed in 1784 (25 George III).

The Palace at Kew was the residence of Augusta Princess of Wales, mother of George III, and the Observatory attached to it fell into disuse. Accordingly, when the Transit of Venus occurred in 1769, facilities for observing it at the old Kew Observatory did not exist. This was pointed out to King George III (apparently by Dr. Demainbray), and he gave orders for the erection of an Observatory in the Old Deer Park, the architect being Sir William Chambers. This was known as the "King's Observatory," and in a paper* by the late Major-General Gibbes Rigaud, it is further styled "the King's Observatory at Kew." Dr. Evans, however, calls it "The Royal Observatory," and speaks of it as being at Richmond.

* "Dr. Demainbray and the King's Observatory at Kew."—"The Observatory," October 2nd, 1882.

The first Superintendent of the Observatory was Dr. Stephen Charles Triboudet Demainbray, descended from parents who had fled to London from France on the Revocation of the Edict of Nantes. This gentleman, after a varied career as a lecturer on science in various universities and institutions in these islands and in France, had settled in London as instructor in science to the King before his accession, and subsequently to Queen Charlotte. One point in his career shows the estimation in which he was held in France. I gather from General Rigaud's paper, "In France (although not of the religion of the country) he was received as an 'Associé Ordinaire' and member of the Royal Academy; the only instance of a declared Protestant not being placed on the list termed 'la Liste Etrangère.'"

FIG. 2.—THE KEW OBSERVATORY FROM THE SOUTH-WEST.
Latitude 51° 28′ 6″ N.
Longitude 0° 18′ 47″ W.

When the new Observatory was finished, Dr. Demainbray adjusted the instruments there in time to make the Transit observation, and was its Superintendent until his death in 1782.

George III frequently attended at the Observatory, and procured the best clocks and watches that could be made and placed them in the Observatory, so that by daily observations of the sun when passing the meridian, the time was regulated, and for many years the accurate time for the regulation of the clocks in both Houses of Parliament, at the Horse Guards, St. James's, and elsewhere, was taken from the King's Observatory, before the accommodation was so well and publicly afforded as it is at present from the Royal Observatory at Greenwich. The clock which was the principal timekeeper at the

Observatory is now at the Patent Museum, South Kensington (No. 1426), and is going well. It bears the following inscription :—

"This clock was made by Benjamin Vulliamy, Clock-Maker to the King, for his Majesty George III, by whom it was used in his private observatory at Kew. It was successively the property of their Majesties George IV and William IV, of H.R.H. The duke of Sussex, and of their Majesties Ernest, King of Hanover, and George, King of Hanover, by whom it was given to Frances Moulton, widow of Benjamin Lewis Vulliamy, eldest son of the maker, April 18th, 1854."

His Majesty King George III, with the assistance of Dr. Demainbray, and his son the Rev. Stephen Demainbray (who held and superintended the Observatory, as the astronomer, for upwards of fifty-eight years after his father's death) procured a large collection of instruments, models, &c., besides a large apparatus for experiments in all branches of natural philosophy, as also a very valuable natural history collection. In addition to these, there was a collection of minerals from the Hartz mines; but these were afterwards given by King George IV to the British Museum.

The Observatory was for many years an object of great interest to King George III, and the Rev. S. Demainbray was for a length of time the teacher of the younger members of the King's family, who attended at the Observatory for his lectures on astronomy, electricity, &c. King William IV also took great interest in the Observatory, and frequently visited it.

At the time of the transfer of the Observatory to the British Association, Mr. S. Demainbray retired on a pension, and he died in July, 1854, at the age of ninety-five years.

During the latter part of the fifty-eight years in which he superintended the Observatory he was assisted by his nephew, Stephen Peter Rigaud, Esq., Savilian Professor of Astronomy at Oxford, and Radcliffe Observer. This gentleman took charge of the Observatory during the Oxford vacations, and thus enabled his uncle to reside during those periods on his living in Wiltshire. The King's Observatory lasted, therefore, for seventy-one years, *i.e.*, from 1769 to 1840.

The Observatory itself was at one time in charge of a curator named John Little, who was hanged in 1795 for the murder of two old people in Richmond to whom he owed money, and who was strongly suspected of having murdered a carpenter named Stroud, who was discovered in the principal or octagon room of the Observatory, the body lying under an iron vice. The St. James's Chronicle, in August, 1795, in giving an account of Little's execution, says, "from his civil deportment he was in general the only attendant on His Majesty when he walked in the gardens." The inquest on Stroud at a previous date had resulted in a verdict of accidental death.

We find in a French book, Simond's "Voyage en Angleterre," 8vo,

Paris, 1817, the account of an amusing episode in the astronomical studies of George III during the later years of his life.

Extract from Simond's "Voyage en Angleterre." Paris, 8vo, 1817.

"Le roi aime l'astronomie, et a un Observatoire dans un petit parc à Richmond, appelé "The King's Paddock." Il y a un grand télescope de Herschel; un instrument des passages (transit) de huit pieds de long, à travers lequel nous observâmes passant le meridien; un instrument vertical de douze pieds, pour les observations au zenith: un mural de huit pieds de rayon; un télescope equatorial, et plusieurs autres instrumens moins considerables; quelques modèles de machines, entre autres, une pour déterminer la pression latérale des voûtes; une collection de mineraux, et un cabinet d'instrumens de physique.

"Sa Majesté étant venue à l'Observatoire, il y a quelques années, pour observer une occultation de planètes, un daim poursuivi de Windsor, traversa la rivière, franchit les palissades, suivi de toute la meute, et vint se laisser prendre au pied de l'Observatoire, précisément au moment de l'observation.

"Je demandai si l'attention de Sa Majesté s'était montrée superieure à cette interruption. On me répondit qu'un nuage, malheureusement survenu précisément au même instant, avait rendu l'observation impossible, et qu'autrement rien n'aurait pu l'en distraire."

The following would appear to be a correct account of the incident somewhat romantically treated in the above, as it was narrated by the late Sir James South to Dr. Balfour Stewart:—

One day Sir James was at the Observatory with the King (George III) when they saw the stag hunt from Windsor approaching, and ascended to the roof to watch it. Concealed by the parapet His Majesty pointed out to Sir James the different gentlemen following the hounds, and at the royal dinner in the evening the King created considerable amusement by assigning to the guests the relative places they each occupied in the hunt, as they were unable to imagine what position of vantage His Majesty had occupied during the proceedings.

With this the record of the first period of the Observatory comes to a close, and the building passed into the management of the British Association for the Advancement of Science, for the space of thirty years, till August 1871. The negotiations, which were carried out in connection with the establishment by H.M.'s Commissioners of Woods and Forests, in the first instance with the Royal Society, and in the second with the British Association, cannot be better described than in the following Memorandum drawn up in 1871 by Sir Charles Wheatstone, who himself, with Sir Edward Sabine and Mr. J. P. Gassiot, was among the original subscribers to the undertaking in 1842. The same three gentlemen, as Members of the Committee,

continued their active superintendence of the Observatory during the whole period of its connection with the British Association.

It will be seen that throughout this memorandum it is termed the Kew Observatory in all official documents.

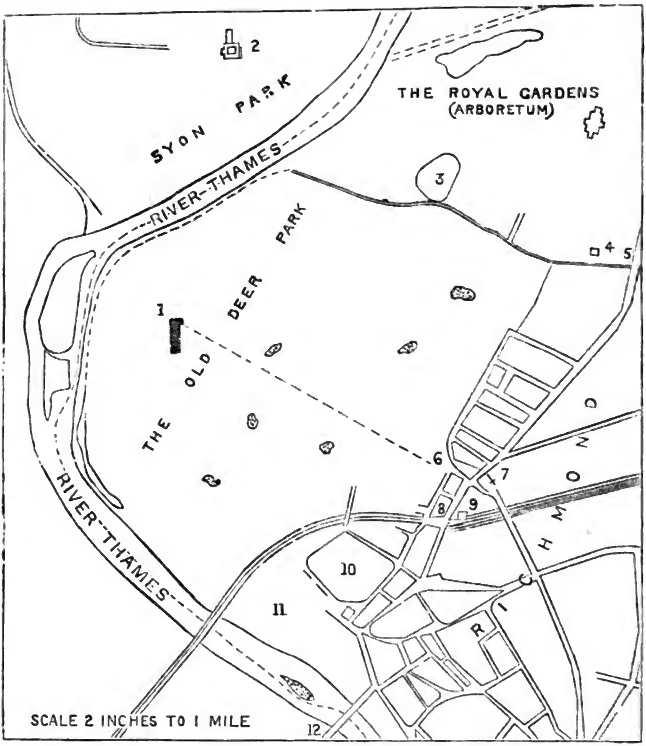

Fig. 3.—The Kew Observatory and Vicinity, 1885.

1. The Observatory and Garden.
2. Syon House.
3. The Queen's Cottage.
4. The Pagoda.
5. The Lion Gate.
6. The Entrance to the Observatory (Fuller's Gate).
7. St. John's Church.
8. Richmond Station (Old).
9. " " (New).
10. Richmond Green.
11. Richmond Palace (Site of).
12. Richmond Bridge.

HISTORICAL REMARKS BY SIR CHARLES WHEATSTONE.

In 1841 the Government came to the determination of no longer keeping up the Observatory and Museum established by His Majesty George the Third in the Old Deer Park at Richmond. In consequence of this resolution, the Curator and the Reader in Natural Philosophy, who had for many years been attached to the building, were pensioned off, and the valuable contents were distributed to the Armagh Observatory, the British Museum, King's College, London, the College of Surgeons, and some members of the Royal Family. The building being thus dismantled and vacant, was applied for by the Council of the Royal Society, on the recommendation of the Committee of Physics and Meteorology, as appears from the following Minutes of June 24, 1841:—

"Mr. Daniell reported, on the part of the Committee of Physics, that they had passed the following resolution, viz.:—

"'The Committee, understanding that the building formerly occupied as the Observatory at Kew is disposable, and may be had if applied for, and having frequently experienced the want of such a building for various scientific purposes, recommend to the Council that an application be made to secure it for the Royal Society.'

"Resolved,—That this recommendation be adopted; and that the President be requested to make application to the proper quarter for the possession of the building in question."

An application was accordingly made, and the possession granted to the Royal Society; consequent on which the following resolution was recorded on November the 11th of the same year:—

"Resolved,—That it be referred to the Committee of Physics to consider and report to the Council to what specific scientific purposes it would be desirable to appropriate the building formerly occupied by the Observatory at Kew, in case of a grant of that building being made to the Society by the Government; and what would be the probable annual expense of applying it to such purposes."

On January the 10th, 1842, a communication was received by the Secretary from the Office of Woods and Forests, requesting to be informed when the Society would be prepared to take possession of the building, as it had long since been cleared and ready to be delivered up when required. The following resolution was thereon recorded:—

"That the Secretary inform Mr. Milne, in answer to his letter, that the Council having referred the matter in question to the consideration of a Committee, beg to defer their answer till they receive the report of that Committee, which they expect will soon be prepared."

The report called for was read on February 10, 1842, and was as follows :—

"The Sub-Committee report that, from the peculiar restrictions as to access and inhabitancy, and other circumstances affecting the proposed grant of the Kew Observatory to the Royal Society, they do not consider that any regular and systematic course of physical observations at present devisable could be therein advantageously made by the Society, or by any observer under their immediate appointment and direction; but that nevertheless they consider that such a building so held might, if occupied for safe custody by a proper person, be highly available for many useful scientific purposes, which have long been desiderata as part of the establishment at Somerset House; such as, among others, the following :—

"1. As a depository of instruments and other property of the Royal Society, either not in use, or for which, for general or special reasons, Somerset House may not be regarded by the Council as an advantageous place of deposit.

"2. As a station for occasional observation and comparison of pendulums, either returned from abroad, or about to proceed on voyages, as also for affording foreigners wishing to compare pendulums an opportunity of so doing.

"3. As a station for trial and comparison of magnetical apparatus, and affording to observers desirous of acquiring a knowledge of the nature and use of such apparatus, an opportunity for conveniently doing so, and of obtaining a practical knowledge of the system of magnetical observation recommended by the Society.

"4. As a station in which many occasional phenomena might be advantageously observed by Fellows of the Society, or others, on permission obtained from the President and Council, such as, for example, concerted observations of shooting-stars, &c., or in which the phenomena of what has been called 'magnetic storms,' or unusual magnetic disturbances might be witnessed, and their particulars attended to by observers desirous of so doing, without interfering with observations regularly in progress at official stations, or with a view to other circumstances in their phenomena than what may be ordinarily observed at such stations; it being considered that such 'storms' are sometimes of considerable duration, and may become known to exist by direct communication from Greenwich or other regular observing station.

"5. As a proper place for the trial of physical apparatus and occasional physical experiments, for which no convenience exists at Somerset House; such as may either be proposed by scientific committees, or be undertaken by individuals, on permission duly obtained from the President and Council.

"6. As a proper place for the comparison of standards of every

description in which general steadiness of temperature and structure, light, retirement, and security may be especially needed.

"As regards the probable cost of such an establishment to the Society, it appears to include essentially:—1st, the wages of a man who should reside in the house, and take care of it; 2nd, such repairs as may from time to time be required, should Government not take on itself the charge of maintenance of the structure; 3rd, the interest of money expended in fitting it for the purposes in view; and 4th, the taxes and rates which may be incident on its occupation.

"As to the first of these items, the Sub-Committee consider that an amount of 10*s*. 6*d*. per week, 27*l*. 6*s*. per annum, would suffice with fuel.

"The building has been represented to them as being in excellent repair, and not to have cost 5*l*. annually for some time in its maintenance in that state; nor are they without reason to hope that such an understanding might be had with the proper authorities as would strike out this item in future.

"It has also been stated to them, that among the fixtures of the buildings are included an ample provision of glass cases and other receptacles well adapted for the preservation of instruments and books, and which have heretofore served for the preservation of the very valuable collections lately presented by Her Majesty to King's College, London, and which would materially alleviate the charges incidental on this head.

"J. F. W. Herschel.
"C. Wheatstone.
"Edward Sabine."

On the 10th of March, however, the Council of the Royal Society rejected the gift they had previously solicited, in the following resolution:—

"Resolved,—That, on full consideration of the report of the Committee of Physics, including Meteorology, presented at the last meeting of the Council, it does not appear to the Council to be expedient for the Society to occupy the Observatory at Kew; and that the Treasurer be requested to make known this decision to the Commissioners of the Woods and Forests, expressing at the same time their thanks for the courtesy and attention paid to the suggestion of the Council on the subject."

The building having thus become again unappropriated, a number of Fellows of the Royal Society and Members of the British Association, desirous that it should be retained for the purposes of science, recommended that an application should be made for it in the name of the British Association, and entered into a subscription for the purpose of promoting the objects stated in the following prospectus.

The subscription was headed by donations of 15*l.* each from the Marquis of Northampton and Lord Francis Egerton as Presidents respectively of the Royal Society and the British Association; and the following gentlemen entered their names for sums varying from 5*l.* to 10*l.* each:—

Sir John Herschel, Sir John Lubbock, Sir Charles Lemon, Sir H. de la Beche, Sir John Rennie, Lieut.-Col. Sabine, Capt. Chapman, The Rev. Dr. Buckland, Dr. Arnott, Dr. Fitton, Prof. Daniell, Prof. Graham, Prof. M'Cullagh, T.C.D., Prof. Wheatstone, R. I. Murchison, Esq., J. Davies Gilbert, Esq., H. F. Talbot, Esq., J. Taylor, Esq., G. Rennie, Esq., J. Evans, Esq., J. P. Gassiot, Esq., and R. Napier, Esq.

The views of these gentlemen were expressed as follows:—

"It has frequently been the subject of regret that there does not exist in this country any of those facilities for the encouragement and advancement of physical science which have been so liberally afforded by the Governments of other nations.

"The Continental philosopher, when he for the first time visits our shores, finds to his great surprise that in the metropolis of this great empire there is not one collection of physical instruments which can afford the slightest idea of the present advanced state of scientific investigation. A few of our establishments for public instruction, indeed, are provided with apparatus for exhibiting the most usual results of experimental philosophy to classes of learners at public lectures, and there are two or three public exhibitions at which some of the most popular and practical results are shown, but the most valuable instruments of scientific researches, particularly those by means of which alone accurate quantitative results can be obtained, are nowhere to be found. It is to be hoped that ere many years have passed a National Physical Museum worthy of the name may be established in London, but as this is an object which we cannot expect to see realized by the private co-operation of individuals, it is proposed to carry into present effect a plan of a more limited nature, which will supply some of the most obvious and urgent existing wants otherwise unprovided for, and which does not seem beyond our means of accomplishing.

"It is proposed to establish, in connexion with the British Association, a Physical Observatory. The useful purposes to which it is intended to apply the Building at Kew, to be placed at the disposal of the British Association, are, among others, the following:—

"1. It will be a repository for, and place for occasional observation and comparison of the various instruments which the recent discoveries in physical science have suggested for improving our knowledge of meteorology, &c., in order that their relative advantages and defects may be ascertained.

"A great number of very ingenious instruments have been invented

on the Continent within the last twenty years, which are practically unknown in this country, but which, if properly understood, verified, and brought into use, would lead to valuable results.

"At the present moment a most important and widely extended series of simultaneous observations in different parts of the world is being carried out, and to this object the British Government and East India Company have munificently contributed. With the exception of the magnetical instruments, none are at present employed for these observations but those which have been long known and verified; this has arisen from want of proper knowledge of the means of constructing, using, and interpreting the results of the new instruments. Did we possess this knowledge, the harvest to be gained from the system of combined observations, now in operation, might be more than doubled, with scarcely any increase of expense.

"2. A repository and station for trial of new instruments, having the same objects as the above in view, which may be proposed in this country. Among instruments which have been proposed, and which will probably not be constructed and brought into use without the assistance which an Institution like this alone can afford, may be mentioned: a universal meteorograph, which will accurately record half-hourly indications of various meteorological instruments, dispensing entirely with the attendance of an observer; an apparatus for recording the direction and intensity of the wind simultaneously at various heights above the earth's surface; an apparatus for telegraphing the indications of meteorological instruments carried up in balloons or by kites, to an observer at the earth's surface.

"3. As a station to which persons, willing to become coadjutors in the system of consentaneous observations, may bring their instruments for the purpose of comparison with the standard instruments there deposited. Attention need not be called to the increased value of observations made with instruments thus properly compared.

"4. As the depository of a complete set of the magnetic instruments at present in use in the various magnetic observatories, in order that any person desirous of so doing may understand their construction and acquire their use. The only magnetic observatory in England is at Greenwich, and the instruments, being in constant use, cannot be employed for the purposes here mentioned.

"5. A complete series of apparatus for experiments on atmospheric electricity. For such investigation the locality is peculiarly adapted. Nothing of the kind at present exists in England, and yet there is no subject in meteorological science for which so much remains to be done.

"6. One of the rooms to be fitted up as an optical chamber with a Heliostat, Fraunhofer's prismatic telescope, photometers, &c., principally for the purposes of optical astronomy, a subject at present totally neglected.

"7. As complete a collection as can be gathered together of the measuring instruments employed in the various branches of physical science, for the purpose of obtaining accurate quantitative results.

"The facilities which such a collection would give to original investigation do not at present exist in this country.

"It is not at present recommended that any expenditure of the funds should be made for apparatus intended merely to exhibit the necessary consequences of established laws."

A successful application was then made by the authorities of the British Association for the possession of the building; and at the Meeting at Manchester in June, 1842, the President of the year, Lord Francis Egerton, thus alluded to this gift of the Government in his opening address:—

"I have been speaking of matters for some time past in progress and notorious to all who have taken an interest in your proceedings. They are gratifying as proofs that the impulse of this Society has been communicated and felt in high quarters. It is surely desirable that under any form of Government, the collective science of a country should be on the most amicable footing with the depositories of its power; free indeed from undue control and interference, not dangling in antechambers, nor wiping the dust from the palace staircases, uncontaminated by the passions and influences with which statesmen have to deal, but enjoying its good will and favour, receiving and requiting with usury its assistance on fitting occasions, and organized in such a manner as to afford reference and advice on topics with respect to which they may be required. * * * The most recent instance I cannot omit—I mean the important accession to the means of this Society of a fixed position, a place for deposit, regulation, and comparison of instruments, and for many more purposes than I could name, perhaps even more than are yet contemplated, in the Observatory at Kew. This building was standing useless. The Council of the Association approached the throne with a petition that they might occupy it, and I am happy to say that the sceptre was gracefully held towards them; and I think this transaction a fair instance of that species of connexion between science and government which I hope may always be cultivated in this country. I am informed that the purposes to which this building is readily and immediately applicable, are of an importance which none but men advanced in science can appreciate. You will hear further of them in the Committee Recommendations." C. W.

KEW OBSERVATORY UNDER THE BRITISH ASSOCIATION.

This period naturally divides itself into two intervals, the first of ten years, 1842 to 1851, when Mr. (afterwards Sir Francis) Ronalds, F.R.S., acted as Honorary Superintendent. In 1852, the second period of twenty years commenced, and the Kew Committee first appeared as actually taking control of the establishment, with Mr. John Welsh, F.R.S., as Superintendent. The Committee had been formally constituted in 1849, as shown by the following extract:—

Extract from the First Minute Book of the Kew Committee of the British Association.

"At a Meeting of the Council of the British Association, held at 6, Queen Street Place, London, October 25, 1849—

"Present:

Sir Charles Malcolm, K.C.B., in the Chair, succeeded by Robert Hutton, Esq.;

W. J. Hamilton, Esq.;	Dr. Forbes Royle;
Robert Hutton, Esq.;	Lieut.-Colonel Sabine;
Francis Ronalds, Esq.;	John Taylor, Esq.;

It was resolved that the following gentlemen be named as a Committee for visiting and exercising a general superintendence over the experiments and observations to be made at Kew; and that the Council are persuaded that the Members of the Committee will render a good service to science, and will entitle themselves to the cordial thanks of the British Association, if they will regard themselves as Members of a real working Committee:—

Sir J. W. Herschel, Bart.;
J. P. Gassiot, Esq.;
Colonel Reid, Royal Engineers;
Colonel Sykes;
Charles Wheatstone, Esq.;

in addition to the President, Trustees, and Officers of the Association."

The first Report of the Committee, signed by Mr. Gassiot as Chairman, was that for 1853. In 1859 Mr. Welsh died, and was succeeded by Mr. Balfour Stewart, F.R.S., who held office for twelve years, up to August, 1871, when the connection of the Observatory with the Association was terminated, as will be explained subsequently.

The first notice we find in the Reports of the Association as to

the administration of the Observatory was the following resolution adopted at the Manchester Meeting in June, 1842 :—

> "That Professor Wheatstone, Professor Daniell, and Mr. Snow Harris be a Committee for constructing a self-recording meteorological apparatus to be employed in the building at Kew, recently placed by Her Majesty the Queen at the disposal of the British Association, with the sum of 50*l.* at their disposal for the purpose.
>
> "Also that the sum of 200*l.* be placed at the disposal of the Council for upholding the establishment in the Kew Observatory."

In the volume for the next year, 1843, we have the Report of a Committee, consisting of Professor Wheatstone, Mr. Hutton, the General Secretary (Colonel Sabine), and Treasurer (Mr. John Taylor), appointed by the Council to superintend the establishment of meteorological observations at the Kew Observatory. And at the close of the first year the Council report the establishment of the following registries, viz. :—

> "1. An ordinary meteorological record with standard instruments made by Mr. Galloway under the superintendence of Professor Wheatstone.
>
> "2. A meteorological record with self-registering instruments on a new construction by Professor Wheatstone.
>
> "3. A record of the electrical state of the atmosphere.

"Mr. Ronalds' name now appears in connexion with the electrical records, and for the next nine years he acted as Hon. Superintendent.

It is evident that the action of the Council in taking charge of the Observatory was criticised at an early date, for from the Report of the Meeting at Southampton, 1846, we learn that the expediency of discontinuing Kew had been referred to the Council at the previous Meeting, as will be seen by the following extract of a Report from the Council presented to the general Committee, September 9, 1846.

The General Committee at Cambridge having passed a resolution—

> "That it be referred to the Council to take into consideration before the next Meeting of the Association the expediency of discontinuing the Kew Observatory,"

the Council appointed a Committee, consisting of the President (Sir John Herschel), the Dean of Ely, the Astronomer Royal, Professors Graham and Wheatstone, and Lieut.-Colonel Sabine, to collect information on the scientific purposes which the Kew Observatory has served, and on its general usefulness to science and to the Association, from whom they received the following Report :—

"Kew Observatory, May 7, 1846. Present, Sir J. F. W. Herschel,

Bart., the Astronomer Royal, Professors Graham and Wheatstone, and Lieut.-Col. Sabine.

"After an attentive examination of the present state of the establishment and of other matters connected therewith, the following resolutions were unanimously adopted, viz.:—

"That it be recommended to the General Committee that the establishment at Kew, the occupancy of which has been granted by Her Majesty to the British Association, be maintained in its present state of efficiency.

"1. Because it affords, at a very inconsiderable expense, a local habitation to the Association, and a convenient depository for its books, manuscripts, and apparatus.

"2. Because it has afforded to Members of the Association the means of prosecuting many physical inquiries which otherwise would not have been entered upon.

"3. Because the establishment has already become a point of interest to scientific foreigners, several of whom have visited it.

"4. Because the grant of the occupancy of the building by Her Majesty, at the earnest request of the British Association, is an instance of Her Majesty's interest in and approval of the objects of the Association.

"5. Because if the Association at the present time relinquish the establishment it will probably never again be available for the purposes of science.

"6. Because it appears, both from the publications of the British Association and from the records in progress at the establishment, that a great amount of electrical and meteorological observation has been and continues to be made, and that a systematic inquiry into the intricate subject of atmospheric electricity has been carried out by Mr. Ronalds, which has been productive of very material improvements in that subject, and has in effect furnished the model of the processes conducted at the Royal Observatory; and because these inquiries are still in progress under local circumstances extremely favourable.

"7. Because other inquiries into the working of self-registering apparatus, both meteorological and magnetical, are in actual progress at the establishment, and there is a distinct prospect of the facilities it affords being speedily much more largely profited by.

"8. Because the access to the Observatory from London to Members of the Association will shortly be greatly improved by railroads, and because the local facilities and conveniences of the establishment have been very greatly enhanced by alterations in its relations to the Commissioners of Woods and Forests.

Signed. "J. F. W. Herschel, Chairman."

In presenting this Report to the General Committee the Council

requests that it may be understood to convey also the opinion of the Council.

At this early period much of Mr. Ronalds' attention was directed to the subject of atmospheric electricity, and in this investigation he was assisted by Mr. W. Radcliff Birt. At the same time the Hon. Superintendent was far from neglectful of other branches of physical research, and he devoted much of his rare mechanical energy to the invention and perfecting of the photographic processes for the registration of meteorology and terrestrial magnetism, with which the name of the Kew Observatory has been permanently associated.

In the course of the year 1850 magnetic observations were commenced with instruments provided by Lieut.-Colonel Sabine, and Mr. John Welsh was appointed by the Committee as Assistant, Mr. Birt having left the Observatory; and in the following year we notice the first commencement of the Verification Department, which has subsequently become such an important feature of the Kew operations—the purchase of a standard thermometer by Regnault, with the intention of employing the latter instrument in the verification of thermometers made by artists in this country—and the procuring of M. Regnault's apparatus for calibrating and graduating thermometer tubes.

In 1852 the verification work had fairly started, as well as the making of standard thermometers. Mr. Welsh also had made two ascents in a balloon, for the determination of the temperature and hygrometric condition of the air at different elevations, before the date of the British Association Meeting.

In the course of this year Mr. Ronalds left the Observatory to reside on the Continent, and accordingly from this date Mr. Welsh is to be regarded as Superintendent of the Observatory, the first or preliminary stage of the Observatory having come to an end.

In 1853, as already stated, appeared the first Kew Report, signed by Mr. Gassiot as Chairman of the Kew Committee, and in the next year, 1854, we learn that the best form of barometers and thermometers for the use of the Mercantile Marine had been decided on by the Kew Committee at the request of the Board of Trade, and that Mr. Welsh had experimentally tested the marine barometers in a voyage to Leith and back, and in one to and from the Channel Islands.

Also that Sir J. Herschel had suggested the importance of obtaining daily photographic pictures of the sun's disk, and that Mr. Warren de la Rue having reported that the probable cost would not exceed 150*l*., this sum had been procured from the Donation Fund of the Royal Society.

In the same year Robert Beckley was engaged as machinist, on the recommendation of Mr. de la Rue. Two acres of ground were also

secured, contiguous to the Observatory. Welsh's standard barometer dates from 1855, and the introduction of gas to the Observatory from the next year. In the same year Mr. Balfour Stewart was engaged as Assistant, but he only remained for a brief period in that capacity, for in 1857 he was succeeded by Mr. Charles Chambers.

In 1856 Mr. Beckley's modification of Robinson's anemometer was submitted to the Committee, and also a series of monthly determinations of dip and horizontal force was commenced with instruments provided by General Sabine from his Department at Woolwich.

The next important event to be chronicled is that the following Memorandum relative to the re-establishment of self-recording magnetic instruments at the Kew Observatory was submitted to the Committee by General Sabine on July 22, 1856:—

"1. The decennial period in the solar magnetic variations, and its coincidence with a similar period in the frequency and amount of the solar spots, appear to be highly deserving of attention in an Observatory established, as Kew is, for physical researches.

"2. There is reason to suppose that the permanency and regularity in the occurrence of the decennial period in the magnetic variations, and its coincidence with the periodic variation of the solar spots, might be effectually and satisfactorily tested by observations of both classes of phenomena at the alternate periods of maximum and minimum, say, for example, in 1857 and 1858 as the anticipated period of maximum, and in 1863 and 1864 as the anticipated period of minimum, and so forth.

"3. The apparatus constructing under the superintendence of Mr. de la Rue will, it is hoped, fully meet the requirements of the research in respect of the solar spots.

"4. Since the time when the magnetic self-recording instruments belonging to the Kew Observatory were constructed under the direction of Mr. Ronalds, very considerable improvements have been made in the art of photography, and the six months' trial which was made by Mr. Welsh of Mr. Ronalds's instruments has led in several other respects to suggestions for improvements which could not but be expected to be required in instruments of so novel a kind, while at the same time the six months' trial referred to has placed beyond doubt the sufficiency of a properly conducted research by means of self-recording instruments, for the examination of the solar magnetic variations."

The Committee authorized Mr. Welsh to proceed with the construction of the instruments, which were completed at an expense not exceeding 250*l.*, derived from the Government Grant Fund, the instruments remaining at Kew at the disposition of the Council of the Royal Society.

In the same year the Kew Heliograph was completed, and it was arranged that Mr. Welsh should undertake the Magnetic Survey of Scotland, the results of which operation were published by Mr. Stewart in the Report of the British Association for 1859. In 1858 also Mr. G. M. Whipple was engaged as a boy.

In 1859 Mr. Welsh died, and was succeeded by Mr. Balfour Stewart. At this date an instrument had been devised at the Observatory for tabulating the values of the magnetic elements from the magnetograms, and as the staff of assistants at the Observatory was not sufficiently large to undertake these tabulations, General Sabine undertook to have the results tabulated at Woolwich for every hour.

In 1860 the Kew Photoheliograph was taken by Mr. de la Rue, mainly at his own expense, to Spain, for employment at the Solar Eclipse on July 18th. Its use was a complete success, and proved that the red prominences belonged to the sun.

In 1861 Sir W. Thomson's Electrograph was brought into regular operation at Kew, and an account of two years' work with it was prepared and submitted to the Royal Society by Professor J. D. Everett in 1868.

In 1862, at the suggestion of Admiral Fitz Roy, who agreed on the part of the Meteorological Department of the Board of Trade to bear the expense incurred, the Barograph designed by Mr. Ronalds was fitted up, and has been from that date kept in constant operation.

It was also reported that as on account of the inadequate strength of the staff of assistants it was not possible to work the Photo-Heliograph at Kew, it had been in operation at Mr. de la Rue's observatory, at Cranford, since February 7th. The instrument remained in Mr. de la Rue's hands for twelve months, when it was re-erected at Kew.

In 1862 Mr. Stewart communicated to the Royal Society an account of some experiments made at Kew in order to determine the increase between 32° Fahr. and 212° Fahr. of the elasticity of dry atmospheric air the volume of which remains constant; and also of others to determine the freezing-point of mercury.

In the next year Mr. Chambers left the Observatory to enter the India Telegraph Service, and eventually to take charge of the Colába Observatory, Bombay; his place, as Magnetic Assistant, being filled by Mr. Whipple.

In 1865 we hear that the Indian Government having decided that pendulum observations should be made in India, Col. Walker and Capt. Basevi received instruction at Kew. A convenient room for pendulum observations was likewise fitted up in the Observatory, the expense being defrayed from the Government-Grant Fund of the Royal Society; and in this room preliminary observations were made for determining the constants of the two pendulums about to be

used in India. The observations were made by Mr. Loewy, and the results communicated to the Royal Society.

In the same year it is reported that—(1) the solar spectrum was being mapped by the spectroscope belonging to the Chairman (Mr. Gassiot), and at his expense. All the measurements between D and E had been made and completely verified, and a map of this region in accordance with these constructed; the investigation was continued until 1866, at which time about three-fourths of the region between E and F had been mapped. And (2) that Hofrath Schwabe, of Dessau, had very generously placed his valuable and extensive series of sun-pictures at the disposal of the Royal Astronomical Society for the immediate use of the Kew Observatory. The enumeration of spots on the principle followed by Herr Schwabe was then commenced and has been continued up to the present time.

1865 also witnessed the commencement of an investigation, which was continued at intervals for several years, but which cannot be said to have led to definite results as yet. It is thus introduced:—

"At the joint suggestion of Professor Tait, of Edinburgh, and the Superintendent, an ingenious apparatus has been constructed by Mr. Beckley, by means of which a disk can be made to revolve *in vacuo* with great velocity; and a short description of some experiments performed by means of this instrument, with a view of ascertaining whether visible as well as molecular motion is dissipated by a medium pervading space, has been communicated to the Royal Society by the Superintendent in conjunction with Professor Tait."

In 1866 the completion of the Kew Photographic thermograph by Mr. Beckley is announced, and in the same year the General Committee of the British Association adopted the following resolution:—

> "That the Kew Committee be authorized to discuss and make the necessary arrangements with the Board of Trade, should any proposal be made respecting the superintendence, reduction, and publication of meteorological observations, in accordance with the recommendations of the Report of the Committee appointed to consider certain questions relating to the Meteorological Department of the Board of Trade."

This resolution initiated the close relationship between the Kew Observatory and the Meteorological Office, of which the reorganization was then in contemplation: a relationship which has materially modified the course of operations at Kew Observatory in subsequent years.

In 1866 the first set of results obtained from the heliograph were published at the expense of Mr. de la Rue under the following title, "Researches on Solar Physics," by Messrs. Warren de la Rue, B. Stewart, and B. Loewy; first series, "On the Nature of Sun-spots."

This was successively followed in 1867 by the Second Series, entitled "Researches on Solar Physics, Second Series, Area-measurements of sun-spots observed by Mr. Carrington during the seven years 1854–60 inclusive, and deductions therefrom," and in 1868 by Appendix to the second series, "On the Distribution in Heliographic Latitudes of the Sun-spots observed by Carrington." These papers all appeared at Mr. de la Rue's expense, and all bore the names of the same authors.

Two papers were likewise communicated in 1868 to the Royal Society by these gentlemen. The first is entitled "Researches on Solar Physics, Heliographic Positions, and Areas of Sun-spots observed with the Kew Photoheliograph during the years 1862 and 1863." The second, "Account of some recent observations on Sun-spots made at the Kew Observatory."

1867. In the Report of the Kew Committee for this year we have a short account of what steps had been taken by Government with reference to Meteorology, ending with the names of the superintending Meteorological Committee, and stating that on the 3rd of January this Committee had appointed Mr. Balfour Stewart as its Secretary, on the understanding that he should, with the concurrence of the Kew Committee of the British Association, retain his present office of Superintendent of the Kew Observatory.

It was also proposed that Kew Observatory should become the Central Observatory, at which all instruments used by or prepared for the several observatories or stations connected with the Meteorological Department should be verified, the entire expense attendant thereon, or any future expense arising through the connexion of the Observatory with the Meteorological Department, being paid from the funds supplied by the latter, and not in any way from money subscribed by the British Association. These proposals having been submitted to the Kew Committee, they approved of the Kew Observatory being regarded as the Central Observatory of the Meteorological Office, and of Mr. Stewart's holding the office of Secretary to the Scientific Committee superintending that office.

In the same year we are informed that the magnetic curves produced at Kew previously to the month of January, 1865, had all been measured and reduced under the direction of General Sabine, by the staff of his office at Woolwich; and the results of this reduction communicated by General Sabine to the Royal Society in a series of memoirs.

In the course of this and the succeeding year the assistants at the six outlying automatic observatories, in connection with the Meteorological Office, Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia, received their training at Kew, while Mr. Stewart, with the assistance of Mr. Beckley, personally superintended the erection

of the instruments at the several stations. The Report for 1868 contains an account of the work done at Kew as the Central Observatory of the Meteorological Committee, in which it is stated that this work may be divided into four heads:—

(1) The arrangement of the self-recording meteorological instruments, their verification at Kew, and erection at the various stations.

(2) The arrangement of a system of tabulation from the automatic records of these instruments.

(3) The arrangement of a system by means of which the accuracy of the instruments themselves, and of their tabulated records, may be secured.

(4) Work done at Kew as being itself one of the Observatories of the Meteorological Committee.

In 1869 we learn *inter alia* that Mr. Beckley had devised his self-recording rain gauge, an account of which was submitted to the British Association at Exeter. At the close of the year, in October, Dr. Stewart resigned the Secretaryship of the Meteorological Committee, and the relations between the Kew Committee and the Meteorological Office were consequently modified.

KEW OBSERVATORY UNDER THE ROYAL SOCIETY.

The letter from the Secretary of the British Association, communicating the Resolution of the Council of that body, which has been given above, was taken into consideration by the Council of the Royal Society on the 19th of January, 1871. At the Meeting of the Council on the 16th of March of the same year, a letter was read from Mr. J. P. Gassiot, F.R.S., offering to the Royal Society an immediate gift of securities, the proceeds of which were to be devoted to the maintenance of a Central Magnetical and Physical Observatory at Kew. The negotiations for the acceptance of this munificent offer were carried to a successful issue during the ensuing months, and on the 15th of June the following memorandum was entered on the Council Minutes of the Royal Society:—

Memorandum of the general heads of the proposed Deed of Trust of the Fund offered by Mr. Gassiot for maintaining the Kew Observatory and carrying on the magnetic, meteorological, and physical observations there:—

"Securities representing 10,000*l.* are proposed to be given to the Royal Society by J. P. Gassiot, Esq., F.R.S., upon trust, for the purpose of assisting in carrying on and continuing magnetical and meteorological observations with self-recording instruments, and any other physical investigations as may from time to time be found practicable and desirable, in the Kew Observatory, in the Old Deer Park, at Richmond, Surrey, belonging to Her Majesty's Government; or, in the event of that Government at any time declining to continue placing that building at the disposition of the Royal Society, then in any other suitable building as the Council of the Royal Society may determine.

"The Observatory and the income of the Trust Fund are to be under the entire control and management of a Committee to be from time to time appointed by the Council, for the time being, of the Royal Society.

"The services of such Committee (like those of the present Meteorological Committee nominated at the request of Her Majesty's Government) are to be gratuitous.

"The income is to be paid to the Committee (to be by them applied generally towards continuing and maintaining the Observatory and providing the expenses of conducting the observations and investigations), also for any repairs to the Observatory building and premises, or for repair or improvement of the present instruments, or for providing new instruments, as the Committee may from time to time

deem expedient, and generally for carrying out the objects of the Trust as may be from time to time determined by the Committee.

"An annual statement of receipts and expenditure is to be prepared by the Committee with any report that the Committee may from time to time deem to be desirable, and the same are to be presented to the Royal Society, and the report is to be published in the Proceedings of the Royal Society, or in such other form as the Council may from time to time direct.

"If, by reason of the Kew Observatory, or any other such equivalent Observatory being at any time discontinued, the observations should cease to be recorded, and the investigations cease to be made for (say) twelve consecutive months, the Trust Funds, with all accumulations (if any), are to be paid over to the person who shall, for the time being, be, *de facto*, the Treasurer of the London Middle-Class School incorporated by Royal Charter, dated 12th of June, 1866, by the name of "the Corporation for Middle-Class Education in the Metropolis and suburbs thereof," to the intent that the same may be applied for the use and benefit of that Corporation as it may think fit and as a part of its general funds, and the receipt of such officer is to be a sufficient discharge to the person's paying over the same.

"Power to the Royal Society to enlarge such period of twelve months for a further period of not exceeding (say) two years, in order to give time for obtaining a site and constructing a new Observatory, with power to apply the current income, and any accumulations thereof, in aid of those purposes.

"Also, give power to the Royal Society to direct some other charitable disposition of the Trust Fund, in case, at the time of the failure of the Trust as to the Observatory, the London Middle-Class School shall have ceased to exist as a corporation.'

The Deed of Trust was duly prepared and submitted, and was sealed with the corporate seal of the Society on the 29th of June, 1871.

The Kew Committee of the Royal Society was at once appointed, and consisted of the then existing Members of the Meteorological Committee of the Royal Society.

The Observatory was handed over to the Society by the British Association at its Edinburgh Meeting.

Mr. Samuel Jeffery, formerly in charge of the Magnetic Observatory at Hobarton, was then appointed Superintendent, while Mr. R. H. Scott undertook the duties of Honorary Secretary to the Committee, which he continued to discharge for six years until 1877, by which time the future success of the undertaking was fully secured.

Mr. Jeffery held the office of Superintendent until February, 1876, when he was succeeded by Mr. G. M. Whipple, B.Sc., who, as appears from the preceding pages, had been connected with the Observatory for eighteen years.

During the period of the connection of the Observatory with the British Association the expenses incurred in carrying out the various experiments and researches as detailed in the foregoing pages were considerable. These were met by grants from the Government Grant Fund or the Donation Fund of the Royal Society, or from private sources.

During the last fourteen years, however, since the establishment has been under the management of the Royal Society Committee, the necessity for greater economy has been recognised, and the operations have been carried on without assistance from the Funds above mentioned. The administration has, at the same time, been so satisfactorily conducted, and the receipts for the verification of instruments have so largely increased, that the Committee have been enabled, out of the surplus balances at their disposal annually, to provide important and costly additions to the instrumental equipment of the Observatory, such as the complete refitment of the magnetographs, and the purchase successively of a Galton's Thermometer testing apparatus—of a hydraulic press for deep sea thermometers—of a new cathetometer—and lastly of the entire apparatus for watch-rating.

The only extraneous source of income for the Observatory, over and above occasional small grants from the Meteorological Council for special researches, has been the money so liberally supplied by Mr. de la Rue for the completion of the measurement of the sun-pictures.

The Committee has, however, to regret the gradual disappearance of many familiar faces from the band of men who took prominent parts in the management of the Observatory in former years.

Of Superintendents, the death of Mr. Welsh has already been noticed, and his predecessor, Mr. Ronalds, after receiving the well-merited honour of knighthood for his inventions in Telegraphy, died in 1873 at the age of eighty-five years.

Of the members of the British Association Committee, Col. W. H. Sykes, almost its first Chairman, died in 1870; Sir C. Wheatstone, whose name appears in the very first resolution of the British Association relating to Kew, deceased in 1871; Mr. Gassiot, for eighteen years the chairman of the British Association Kew Committee, and the munificent patron of the Observatory in recent years, died in 1876; while Sir E. Sabine, who had been from first to last identified with the magnetic operations at the Observatory, and might almost be termed the guiding spirit of the undertaking, passed away in 1883, having almost attained the patriarchal age of ninety-five years.

The following are the most important facts which can be gathered from the successive Reports of the Royal Society Committee:—

1872. The Photoheliograph was regularly worked, as in former years,

up to the end of February, 1872, at which epoch the period expired which was originally fixed by Mr. de la Rue for the continuance of the observations at the expense of the Royal Society Government-Grant Fund. The observations were afterwards carried on up to the end of March, with the object of fully including ten years. Since that date eye observations of the sun, after the method of Hofrath Schwabe, have been made in order that the observations for connecting sun-spots with magnetic phenomena might not drop through until photographic records had been taken up on a permanent footing.

In order to furnish the final corrections to the reductions of the sun-pictures, a scale of equal parts, 15 feet in length, designed by Mr. de la Rue, and made at his expense, was, with the sanction of Her Majesty's Office of Works, erected temporarily on the Pagoda at the Royal Gardens, Kew. This was photographed by the Kew Photoheliograph, and so enabled the optical distortion of that instrument to be determined.

Observations were continued for several months, when the apparatus was taken down, and the Photoheliograph lent for two years to the Astronomer Royal for use at Greenwich.

1873. A series of experiments were commenced and carried on for a space of nearly two years at the expense of the Meteorological Committee, at the Pagoda in the Royal Gardens, to test the influence of height above the ground on the vertical distribution of temperature. The thermometers were placed at three different levels, viz., 22 feet 6 inches, 69 feet, and 128 feet 10 inches above the ground.

In the month of May a request was received from Col. J. T. Walker, F.R.S., Superintendent of the Great Trigonometrical Survey of India, for provision to be made at the Observatory for vibrating pendulums.

In the year 1865 two pendulums lent by the Royal Society for use in India had been vibrated at Kew by the late Capt. Basevi, as explained above; and it was necessary that these pendulums should be vibrated again on their return, and that at the same time two pendulums obtained from the Imperial Academy of Sciences at St. Petersburg should also be vibrated.

The Committee at once complied with the request; and at the expense of the Indian Government preparation was made for the experiments in the south hall on the basement story, by removing for a time the apparatus for testing sextants, and building up from the foundation-arches two solid isolated supports for the Russian clock and pendulum.

Capt. Heaviside, R.E., the officer charged with the duty of making the pendulum experiments, arrived in England in July, and, finding all the arrangements satisfactory, at once commenced his operations.

Endeavours were made, in connection with the arrangements just mentioned, to obtain an electrical time communication between Kew

and the Royal Observatory at Greenwich; but the proposal failed of success.

The experiments were continued until the end of May, 1874, when the apparatus employed, with the exception of the Russian pendulums and their accessories, was, at the request of the Secretary of the Royal Society, received at Kew for storage.

1874. The Magnetograph instruments were dismounted in January, 1874, for the purpose of thorough examination and readjustment. The necessity for this measure is obvious, when it is remembered that the instruments had been in unintermitted action for the period of fifteen years.

The scale-values were accordingly redetermined, and the instruments handed over to Mr. Adie for examination and repair. They were returned and remounted in May, but were not at that time set in continuous action, inasmuch as it was intended that the automatic records should be suspended for the entire year, so as to commence a new series of observations with the year 1875.

In this year an improved automatic Electrograph, invented by Sir W. Thomson, was erected and set in operation.

In the year 1873 a grant had been obtained from the Government-Grant Committee for the purpose of carrying on a series of experiments on the constants of Robinson's Anemometers; and a piece of ground in the Park was rented. Several anemometers, of various constructions, were erected therein.

The experience of a few months was sufficient to show that the exposure in the Park was not nearly sufficiently open to afford facilities for testing the instruments at any but very low velocities, and not satisfactorily even in such cases. Application was therefore made to the Secretary of the Crystal Palace Company for permission to employ a rotary machine driven by steam-power, so as to be able to vary the velocities at pleasure.

Consent having been most freely given, the experiments were commenced, and the instruments tested at various velocities up to about thirty miles an hour, the highest attainable by the apparatus.

A paper on the results of these experiments was laid before the Royal Society by Prof. Stokes in 1881 (Proceedings Royal Society, Vol. XXXII, p. 170).

1875. The year was marked by the recommencement of regular work with the Magnetographs. The instruments were set in action on the 1st of January, 1875, and therewith the second series of continuous photographic records of magnetic phenomena was inaugurated.

The principal constants employed in the computations for the Tables used in the reduction of the monthly absolute observations which had been determined by Mr. Welsh were also re-examined. A memorandum containing the results of the observations in question for the twelve

months ending September 30th, 1875, was prepared and appended to the Report for the year, and has formed a feature of every subsequent annual report.

The Registering Sunshine Recorder invented by the late Mr. J. F. Campbell, of Islay, F.G.S., which had been in operation for about twenty years at the Office of the Local Government Board, 8, Richmond Terrace, Whitehall, was transferred to Kew, and set in action at the Observatory. The instrument consists of a glass sphere and wooden bowl, and the effect is measured by the amount of wood charred by the sun's action in the course of six months. Experiments were set in progress at the Observatory to obtain a satisfactory daily record of the duration of the sun's heating action by a similar method.

1876. In the previous year Mr. C. S. Peirce, of the United States Coast Survey, had made through the Admiralty an application to the Royal Society to swing his pendulums at the Observatory. Permission was at once granted, and Mr. Peirce arrived at Kew in June, 1876, and as soon as some necessary fittings had been put up in the Pendulum Room he erected his apparatus, and made a complete series of vibrations.

In 1876, at the request of the Editor of the *Times*, and at the expense of that journal, the Staff commenced the preparation of a weekly diagram showing the traces of the self-recording meteorological instruments on a reduced scale, together with an epitome of the general features of the weather. This has appeared without intermission from that date.

The examination and checking of the work of the self-recording Observatories of the Meteorological Committee was discontinued in November. This change of arrangements involved a considerable reduction in the amount allowed by the Meteorological Office to Kew, as its central Observatory.

1877. Sir Edward Sabine having brought the discussion of the Magnetical Observations carried on under his superintendence in all parts of the globe for a period of nearly forty years, to a close in a final "Contribution," presented to the Royal Society (No. XV, Phil. Trans., Vol. 167), represented to the War Office that he was able to dispense with the further services of the two Sergeants of the Royal Artillery who had acted as his clerks, and had been in constant attendance at Kew since November, 1871. These men were in consequence withdrawn on the 31st of March.

The documents deposited in Sir E. Sabine's late office were presented by the War Office to the Royal Society, and, in conformity with instructions received from the Council, are retained in the custody of the Observatory. A detailed list of these documents and papers has been prepared, and a selection made of all those relating to marine observations, which, at the request of the Hydrographer, were transferred to the Hydrographic Department of the Admiralty.

A system was organized for etching a "Hall-mark," as in the annexed figure, upon all Thermometers which have been verified.

The Astronomer Royal having courteously offered the Committee every facility for a suggested comparison between the Greenwich and Kew Standard Barometers during April and May, a number of carefully selected Portable Standard Barometers were conveyed to and fro between Greenwich and Kew on three separate occasions, and a large number of comparative readings were obtained by the Superintendent and Messrs. Baker and Foster.

A complete detailed account of the experiment was drawn up, and laid before the Royal Society, February 7th, 1878. (Proceedings, Vol. XXVII, p. 76.)

A Hydraulic Press especially constructed for the purpose of subjecting Deep Sea Thermometers to pressures similar to those they experience when sunk to great depths, was erected in the workshop. It is capable of exerting a strain equal to 4 tons on the square inch.

1879. Experiments were commenced at the Observatory, with the view of determining the relative merits of different patterns of thermometer screens, and continued for upwards of two years. For this purpose there were erected on the lawn a Stevenson's screen, of the ordinary pattern, and a large wooden cage, enclosing a Wild's screen, of the pattern employed in Russia. Each of these screens contained a dry and a wet bulb thermometer, and a maximum and minimum, all of which were read daily, at 9 A.M. and 9 P.M., their indications being compared with those of the thermograph at the same hours.

A discussion of the twenty-eight months' observations was subsequently made by Mr. Whipple, and the results published by the Meteorological Council.

1880. A Sub-Committee, which had been appointed in 1878 to consider the best means of utilising the records of the magnetographs, reported that it was inadvisable, in their opinion, to proceed with the regular tabulation of the curves, and suggested that attention should rather be directed to their comparison with synchronous curves, taken at other Magnetic Observatories in different parts of the globe, in order to ascertain whether similar disturbances occur at these several stations, and at what time intervals; with a view to the development of the theory of magnetic disturbance.

In order to carry out this scheme, a circular, inviting co-operation on the part of observers provided with magnetographs of the Kew pattern, was issued to the Directors of the following Observatories:— Batavia, Bombay, Brussels, Coimbra, Colába, Lisbon, Mauritius, Melbourne, Potsdam. St. Petersburg (Pawlowsk), San Fernando, Stonyhurst, Utrecht, Vienna, and Zi-Ka-Wei. Replies favourable to

the project were received from all those whose instruments were working under satisfactory circumstances.

An examination of the records for the year 1879 indicated the month of March as that most suitable for the purpose of the comparison. Accordingly, a further request for copies of the Declination curves for that month was issued, and, in response, they were received from Coimbra, Colába, Lisbon, Mauritius, Melbourne, St. Petersburg, Stonyhurst, Toronto, Utrecht, Vienna, and Zi-Ka-Wei.

The comparison of these magnetic curves was undertaken by Professor W. Grylls Adams, who communicated to the Swansea Meeting of the British Association a preliminary account of the principal facts which came to light. This was followed by a second paper printed in the Report of the York Meeting, 1881.

At the request of the Council of the Royal Astronomical Society, the valuable collection of MSS. containing the memorable series of sun-spot observations made by Hofrath Schwabe, of Dessau, during the years 1825 to 1867, which had been deposited in the Library of the Observatory, the first volumes since 1865, was transferred to the Society's Library at Burlington House, London. In order, however, to render the collection of sun-spot observations as complete as possible, and to prevent the total loss of the observations in case of fire, the Committee voted the sum of 90*l*. to defray the cost of making a complete copy of the solar drawings.

This was accordingly done, and accurate tracings made of every one of Schwabe's drawings. These were pasted into blank books, and any important notes were transcribed at the same time.

The Observatory, therefore, now possesses a complete record of the condition of the sun's surface, extending from November, 1825, to the present date.

In **1881**, an application was received from Major Herschel, R.E., F.R.S., by authority of the India Office, for permission to make certain experiments with the invariable pendulums deposited at Kew, and for the loan of the instruments, with their accompanying appliances, with facilities for prosecuting the experiments at the Observatory.

These requests were granted, and in the autumn of the year operations were carried on, both in the Pendulum Room and in the Experimental House at Kew.

On completing his work Major Herschel conveyed the instruments he employed, first to the Royal Observatory, Greenwich, and subsequently to a house near Portland Place, London. Series of observations were made in both those places, with the object of reducing to a common standard the determinations of gravity made by Kater, Airy, Sabine, and others.

On the conclusion of these latter experiments, Major Herschel conveyed the pendulums, clock, &c., to America, where, after making a

series of observations at Washington, he handed them over to the officers of the United States Coast Department. Subsequently Professor C. S. Peirce, of the United States Coast Survey, who had made a series of pendulum observations at Kew and elsewhere in 1876, visited the Observatory in July, 1883, and made a subsidiary series of experiments with a view of determining the flexure of his stand when on the Kew piers.

1882. In April of this year the Chairman announced the completion of the long series of reductions of the Kew Photoheliograph measurements, extending over the period February 7th, 1862, to April 9th, 1872. He stated that the MSS. volumes containing the results had been deposited for reference with the Royal Society, and that the whole of the sun-pictures had been re-measured at the Observatory, and reduced by Mr. A. Marth, F.R.A.S., so as to give the heliocentric longitudes and latitudes of the spots, as well as their areas.

The entire expense attendant on these reductions, which has amounted to the considerable sum of 1,452*l.* 6*s.* 7*d.*, was defrayed by Mr. de la Rue. This amount was in addition to the sums that gentleman had previously disbursed in contributing to the maintenance of the Photoheliograph, and the prosecution of solar research. The total amount of his payments towards this branch of the Observatory work has reached the large sum of 2,071*l.* 15*s.* 4*d.*

In the same year the Committee received very gratifying testimony as to the accuracy of the Standard Thermometers constructed at the Observatory. In a paper contributed to the "American Journal of Science," Dr. Leonard Waldo, of the Winchester Observatory, Yale College, U.S.A., remarks that after a critical examination of three Kew Standard Thermometers, in which every degree was separately measured, entailing no less than 2,300 micrometer readings, he came to the conclusion that their errors are practically insensible, and too small to be detected with certainty.

Professors Thorpe and Rücker also tested very minutely three similar instruments made for them at Kew. In a paper read at York before the British Association, Professor Rücker stated, "In no case would the calibration error in the determination of a difference of temperature have amounted to 0·02° C. It may therefore be concluded that Welsh's method, as applied at Kew to selected tubes, and with a measuring instrument of great accuracy, is capable of giving first-rate results. The errors which remain when it has been applied are so small that they may be neglected in all cases but those where the thermometers are to be used under the most favourable conditions, *i.e.*, with the stem of the same temperature as the bulb, &c. This satisfactory conclusion is confirmed by the fact that Professor Rowland has recently stated that the calibration of the Kew thermometer used by him in his research on the mechanical equivalent of heat was practically perfect." (British Association Report, 1881, p. 541.)

1883. In this year the Committee decided to make a trial of a system of watch-rating for the public, and granted 100*l.* for the preliminary expenses. In accordance with a scheme prepared by the Superintendent, they fitted up in the Observatory a first-class burglar- and fire-proof safe for the safe custody of the watches. In the next year's Report we learn that the arrangements for rating watches mentioned in last year's Report had been completed and brought into operation successfully, at a cost of 193*l.*

1884. A second safe having been purchased by the Committee, an apparatus was fitted to it which enables the enclosed watches to be maintained continuously at either high or low temperatures, whichever may be required, and furthermore without being subjected to injury by fumes of gas in the former case.

The following was the constitution of the Kew Committee at the close of the year 1884:—

Mr. W. de la Rue, Chairman.
Captain W. de W. Abney, R.E.
Professor W. G. Adams.
Captain Sir F. Evans, K.C.B.
Professor G. C. Foster.
Mr. F. Galton.
Admiral Sir G. H. Richards, C.B.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-General W. J. Smythe.
Lieut.-General R. Strachey, C.S.I.
Mr. E. Walker.

The staff employed at the same date was as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant and Magnetic Observer.
J. Foster.
H. McLaughlin.
E. G. Constable.
T. Gunter.
With five juniors.

The following may be considered as a summarised statement of the scientific work done at the Kew Observatory since its foundation as a Physical Observatory:—

1. Improvements in the construction and manufacture of instruments for meteorological observations, the production of new ones, and the modification of older forms.
2. The same with regard to magnetic and electrical instruments.
3. The distribution of such instruments over the globe.
4. The ensuring of accuracy of observations by means of the verification and distribution of standard instruments and the instruction of observers.
5. The continuous recording of magnetical and meteorological phenomena.
6. The practical adaptation of photography to scientific purposes, and especially to the continuous registration of phenomena occurring on the sun's surface.
7. The improvements of nautical, geographical, fiscal, physical, and medical instruments by verification, and of horological by watch-rating.
8. Assistance in the prosecution of research in the following sciences:—
    1. *Astronomy.*—De la Rue's, Stewart's, and Loewy's solar researches; Gassiot's spectroscopy.
    2. *Geodesy.*—Pendulum observations—Basevi, Heaviside, Peirce, Herschel.
    3. *Magnetism.*—Sabine and others too numerous to mention.
    4. *Meteorology.*—Ditto, ditto.
    5. *Physics.*—Roscoe's Actinometry; Stewart's ditto.
       Sanitary Institution—Ventilation.
       Greaves, Evaporation.
       Stewart, Dilatation of Air and Theory of Exchange Experiments.
       Stokes, Friction of Gases.
       Loewy, Pressure on Thermometers.
       Anemometrical Coefficients, &c.
    6. *Magnetic Surveys*—Scotch (Welsh); Arctic and Antarctic—Perry's, Thorpe's, &c.; Fort Rae and other Expeditions.

The following Appendices supply particulars as to the operations at the Observatory in successive years:—

A. Physical Constants for Kew Observatory.
B. List of Observatories supplied with Magnetographs.
C. " " " Meteorographs.
D. List of Verifications.
E. Catalogue of Papers relating to Kew Observatory.
F. Annual Statement of Money received by Kew Observatory.

APPENDIX A.

KEW OBSERVATORY, RICHMOND, SURREY.

Latitude 51° 28′ 6″ N. — Longitude 0h. 1m. 15·1s. W. 0° 18′ 47″ W.

Height of Barometer cistern above sea-level		34 feet.	
Magnetic Constants	Mean declination for 1885	18° 25′ West.	
		English Units	C. G. S.
	,, horizontal force for 1885	3·92	0·18
	,, vertical ,, ,, ...	9·53	0·44
	,, total ,, ,, ...	10·30	0·47
Force of gravity...		32·190 feet.	
Length of seconds pendulum		39·138 inches.	

METEOROLOGICAL CONSTANTS.

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Mean.
Mean height of barometer reduced to sea-level, 1871–80	29·961	29·909	29·922	29·840	29·955	29·920	29·914	29·903	29·917	29·866	29·853	29·909	29·906″
Mean temperature of air, 1871–80	39·2	40·8	42·9	47·5	51·5	58·8	62·4	62·1	56·7	49·4	42·5	39·1	49·4°
Mean temperature of evaporation, 1871–80	37·6	38·9	40·1	44·0	47·3	54·3	57·7	57·7	53·5	47·3	40·6	37·8	46·4°
Mean rainfall, 1871–80	2·14	1·68	1·26	2·13	1·79	2·45	2·83	2·29	2·43	2·77	2·20	2·01	2·16″
Mean percentages of bright sunshine, 1880–84	16	19	33	29	39	34	36	39	34	27	25	12	29%

B. List of Observatories supplied with Kew Pattern Magnetographs.

Toronto } Ronalds' pattern.
Madrid }

1. Batavia.
2. Coimbra.
3. Lisbon.
4. United States (?).
5. St. Petersburg (Pawlowsk).
6. Florence.
7. Stonyhurst.
8. Utrecht.
9. Melbourne.
10. Bombay (Colába).
11. Mauritius.
12. Vienna (Hohe-Warte.)
13. Zi-Ka-Wei.
14. San Fernando.
15. Potsdam.
16. Brussels.
17. Nice.
18. United States (Naval Department.)

C. List of Observatories supplied with self-recording Meteorological Instruments on the Kew Model.

	Barograph.	Thermograph.	Anemograph.	Rain gauge.
Valencia	1	1	1	1
Armagh	1	1	1	1
Aberdeen	1	1	1	1
Glasgow	1	1	1	1
Stonyhurst	1	1	1	1
Falmouth	1	1	1	1
Radcliffe	1	1	1	1
Toronto	1	1	—	—
Mauritius	1	1	—	—
Brussels	1	1	—	—
Zi-Ka-Wei	1	1	1	—
Adelaide	1	1	—	—
Japanese Govt.	2	1	—	—
Hong-Kong	1	1	—	1
Bombay	1	1	1	—
Sydney	1	—	—	—
Melbourne	1	1	—	—
Batavia	1	1	—	1

D. LIST OF VERIFICATIONS COMPLETED AT THE OBSERVATORY.

	Thermometers.	Barometers.	Hydrometers.	Standard Thermometers made.
1852–53	—	—	—	70
1853–54	181	23	—	24
1854–55	2520	257	1269	—
1855–56	530	137	100	—
1856–57	1524	278	751	—
1857–58	268	221	150	—
1858–59	911	187	92	—
1859–60	222	173	—	—
1860–61	660	150	8	7
1861–62	282	184	—	—
1862–63	296	130	22	1
1863–64	389	97	—	5
1864–65	420	88	—	6
1865–66	395	126	—	8
1866–67	608	89	—	2
1867–68	1138	78	—	14
1868–69	1153	157	38	12

NUMBER OF INSTRUMENTS VERIFIED, &c., SINCE 1869.

	Thermometers.						Barometers.				Magnetic Apparatus.					Self-Registering Apparatus.						Surveying Instruments, &c.		
	Standards Constructed.	Standards Tested.'	Ordinary Meteorological.	Mountain.	Clinical.	Solar Radiation, Deep Sea, &c.	Standard.	Marine and Station.	Aneroids.	Hydrometers.	Unifilars.	Dip Circles and Fox's do.	Dipping and Magneto-graph Needles.	Compasses.	Deflection Bars, Declinometers, Magnets, &c.	Magnetographs.	Barographs.	Thermographs.	Anemographs.	Rain Gauges.	Anemometers, Scales, and Evaporating Dishes.	Sextants.	Theodolites.	Clinometers, Telescopes, and Sextant Glasses.
1869–70	21	20	851	20	269	—	34	134	12	103	1	4	4	—	1	—	—	—	—	—	—	—	—	—
1870–71	14	20	876	7	417	6	22	76	17	215	—	2	4	—	—	—	—	—	—	—	—	—	—	—
1871–72	12	31	1,219	16	1,395	24	34	73	17	12	4	5	6	—	—	1	—	—	—	1	6	3	1	—
1872–73	9	29	782	52	1,233	—	49	110	20	—	3	5	2	1	—	—	—	—	—	1	1	—	—	—
1873–74	18	22	1,471	32	1,255	37	110	40	10	66	4	6	5	3	6	1	1	1	—	13	14	1	1	1
1874–75	13	64	1,238	20	1,439	272	67	126	21	150	3	7	1	—	5	1	—	1	—	3	6	1	1	—
1875–76	10	36	1,410	34	1,560	311	96	106	28	129	3	5	7	—	3	—	—	—	—	29	20	1	—	—
1876–77	12	57	1,428	22	2,281	177	71	113	25	249	1	3	2	2	2	2	—	—	—	11	5	3	1	1
1877–78	14	47	1,435	16	2,032	199	56	137	29	356	4	7	2	3	—	1	2	3	—	—	1	2	—	—
1878–79	12	67	1,286	43	3,405	88	68	77	51	120	5	6	4	—	2	—	—	—	—	6	2	4	5	—
1879–80	17	94	1,487	68	3,638	244	47	156	21	10	1	6	9	1	—	—	—	—	—	13	17	5	4	—
1880–81	10	60	1,704	40	4,217	118	59	109	34	47	3	7	5	—	—	1	—	—	—	6	13	25	3	237
1881–82	11	166	1,518	69	5,365	243	48	105	30	195	5	10	1	—	5	—	2		—	4	12	36	4	8
1882–83	7	116	1,165	39	7,255	164	45	114	52	59	8	5	—	—	—	1	2	1	1	9	12	55	1	393
1883–84	10	83	1,225	164	8,726	197	44	80	84	1,161	5	9	4	—	6	1		1	—	3	2	64	—	341

E. CATALOGUE OF PAPERS RELATING TO KEW OBSERVATORY.

Adams, W. Grylls—

Comparison of Curves of the Declination Magnetographs at Kew, Stonyhurst, Coimbra, Lisbon, Vienna, and St. Petersburg. *Brit. Assoc. Rep.*, L, 1880, pp. 201–209.

On Magnetic Disturbances and Earth-Currents. *Brit. Assoc. Rep.*, LI, 1881, pp. 463–474.

Beckley, R.—

Description of a Self-recording Anemometer. *Brit. Assoc. Rep.*, XXVIII, 1858, pp. 306, 307.

Birt, W. R.—

Report on the Discussion of the Electrical Observations at Kew. *Brit. Assoc. Rep.*, 1849, pp. 113–199.

De la Rue, Warren—

On the Total Solar Eclipse of July 18th, 1860, observed at Rivabellosa, near Miranda de Ebro, in Spain. *Phil. Trans.*, 1862, pp. 333–416.

Researches on Solar Physics. First Series. On the Nature of Sun-spots. *Amer. Journ. Science*, XLIII, 1867, pp. 179–192.

Researches on Solar Physics. Second Series. Area Measurement of the Sun-spots observed by Carrington during the seven years from 1854–1860 inclusive, and deduction therefrom. *Amer. Journ. Science*, XLIII, 1867, pp. 322–330.

Results of the Observations on Sun-spots made in Kew and in Dessau during the years 1867–68. *Astron. Soc. Month. Not.*, XXVIII, 1868, pp. 44, 45; XXIX, 1869, p. 95.

Sun-spots and general Aspect of the Sun on the day of the Total Eclipse, 18th August, 1868. *Astron. Soc. Month. Not.*, XXIX, 1869, pp. 3, 4.

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F. Annual Statement of Money received for the carrying on of the Kew Observatory from its commencement to November, 1884, first under the British Association, and subsequently under the Royal Society, classified under the different heads whence the various sums were obtained, as specified in the Reports to the two above-named Societies.

	British Association.		Meteorological Office.		Donations.	Verification Fees.	Commissions, Sales and other Sources.	Annual Totals as per Reports.
	Maintenance Fund.	Special Purpose.	Annual Allowance.	Special Work, Postages, &c.				
	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.
1842—1843	133 4 7	...	...	...	...	...	...	133 4 7
1843—1844	117 17 3	56 7 3	...	...	...	...	...	174 4 6
1844—1845	149 15 0	43 17 8	...	...	...	...	...	193 12 8
1845—1846	146 16 7	...	...	...	...	...	...	146 16 7
1846—1847	107 8 6	...	...	...	...	...	...	107 8 6
1847—1848	171 15 11	...	...	...	...	...	...	171 15 11
1848—1849	76 2 5	...	...	...	...	...	...	76 2 5
1849—1850	255 18 0	...	...	...	...	...	...	255 18 0
1850—1851	309 2 2	...	...	...	...	...	...	309 2 2
1851—1852	233 17 8	...	...	...	...	...	...	233 17 8
1852—1853	165 0 0	...	...	...	...	...	...	165 0 0
1853—1854	330 15 4	...	...	...	...	...	...	330 15 4
1854—1855	500 0 0	...	...	...	...	101 0 0	1 16 0	602 16 0
1855—1856	500 0 0	...	...	...	...	221 7 8	14 15 0	736 2 8
1856—1857	350 0 0	...	...	...	...	141 5 0	...	491 5 0
1857—1858	500 0 0	...	...	...	...	110 0 0	...	610 0 0
1858—1859	500 0 0	...	...	...	...	69 12 7	...	569 12 7
1859—1860	500 0 0	...	...	...	*329 12 6	116 9 8	3 8 0	949 10 2
1860—1861	500 0 0	...	...	...	*90 0 0	81 5 0	36 15 8	708 0 8
1861—1862	500 0 0	...	...	...	...	63 19 0	14 0 0	577 19 0
1862—1863	600 0 0	...	...	16 14 3	...	81 12 6	...	698 6 9
1863—1864	600 0 0	...	...	26 2 10	†100 0 0	76 18 0	...	803 0 10
1864—1865	600 0 0	...	...	26 1 5	...	74 0 6	50 0 0	750 1 11
1865—1866	600 0 0	...	...	26 1 5	...	102 0 0	...	728 1 5
1866—1867	600 0 0	...	...	26 0 0	...	114 8 2	24 0 0	764 8 2
1867—1868	600 0 0	...	...	208 0 0	...	97 10 0	...	905 10 0
1868—1869	600 0 0	...	430 0 0	34 1 4	...	157 1 6	...	1,221 2 10

* The Royal Society.

† Mr Gassiot.

ANNUAL STATEMENT OF MONEY received for the carrying on of the Kew Observatory—*continued.*

	British Association.		Meteorological Office.		Donations.	Verification Fees.	Commissions, Sales and other Sources.	Annual Totals as per Reports.
	Maintenance Fund.	Special Purpose.	Annual Allowance.	Special Work, Postages, &c.				
	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.
1869—1870	600 0 0	...	560 0 0	7 10 0	†202 11 7	133 16 0	24 17 1	1,528 14 8
1870—1871	600 0 0	...	541 13 4	80 13 6	*96 6 1	131 6 9	23 11 1	1,473 10 9
1871—1872	300 0 0	...						
	Royal Society.				Mr. de la Rue for Sun-work.			
	Gassiot Trust.	Special Purpose.						
1871—1872	600 0 0	...	767 18 5	3 2 6	20 13 1	125 6 0	267 12 9	2,084 12 9
1872—1873	608 0 7	...	649 7 6	23 16 11	49 9 6	302 15 11	260 14 8	1,894 5 1
1873—1874	498 18 4	...	650 0 0	40 0 6	34 17 1	253 10 9	402 3 10	1,879 10 6
1874—1875	499 19 4	...	650 0 0	38 7 10	100 0 0	368 7 6	436 13 5	2,093 8 1
1875—1876	498 18 4	...	650 0 0	26 7 1	109 13 0	380 8 7	550 11 6	2,215 18 6
1876—1877	497 17 4	...	420 16 8	7 11 11	106 4 6	395 4 1	508 4 11	1,935 19 5
1877—1878	495 15 3	...	400 0 0	15 5 8	125 7 10	486 2 11	976 0 0	2,498 11 8
1878—1879	495 14 0	...	410 8 4	14 15 3	104 1 3	405 7 7	606 3 7	2,036 10 0
1879—1880	496 13 8	...	400 0 0	35 12 9	100 0 0	447 3 0	372 15 4	1,852 4 9
1880—1881	496 13 8	...	4 0 0 0	44 17 2	8 6 8	523 8 5	364 18 5	1,838 4 4
1881—1882	496 14 6	...	400 0 0	35 11 7	...	621 17 0	680 15 0	2,234 18 1
1882—1883	493 11 1	...	400 0 0	9 5 1	15 8 3	615 18 6	551 7 6	2,085 10 5
1883—1884	494 4 10	25 0 0	400 0 0	27 3 11	...	759 4 11	822 7 6	2,528 1 2

* The Royal Society † Mr. Gassiot.

"On the Microscopic Characters of some Specimens of Devitrified Glass, with Notes on certain analogous Structures in Rocks." By DOUGLAS HERMAN and FRANK RUTLEY. Communicated by Professor T. G. BONNEY, D.Sc., F.R.S. Received May 28, 1885. Read June 18.

(Plates 1—4.)

Devitrification is a process which may either take place naturally or be brought about by artificial means. Instances of the former are familiar to us in once glassy rocks which have passed into a felsitic or micro-crystalline-granular condition. The change which has taken place in the conversion of obsidian into felstone is so great that it would not be possible to infer the original nature of the rock, were it not that certain structural peculiarities, often of a very delicate character, are retained. It is, indeed, a most remarkable feature in such rocks that a physical change so complete should fail to obliterate the perlitic structure and the fine streaky markings or fluxion-bands which are common in the vitreous lavas of every age. The microscopic recognition of such structures has of late years added considerably to our knowledge of the rhyolitic rocks and tuffs of Archæan and Palæozoic times, many of which were undoubtedly hyaline rhyolites. Through devitrification their original character has been obscured, and in many instances can only be revealed by the use of the microscope. Although much has been written upon this subject, including Vogelsang's admirable work,* we are still in comparative ignorance of the conditions under which such devitrification has taken place. The experiments of Daubrée,† made upon glass tubes at a high temperature and pressure, in presence of water, resulted in the development of a schistose or foliated structure, corresponding with the cylindrical form of the tubes acted upon: the development in some cases of reticulating cracks, due to contraction, the transformation of the glass into a friable substance with a structure both fibrous and concentric, and also into a hard material with similar structure. Professor Daubrée has likewise in these experiments produced radiating crystalline or spherulitic bodies, microliths, and actual crystals of pyroxene and quartz. The glass upon which he operated contained, in its unaltered condition—

* "Die Krystalliten." Bonn, 1875.

† "Études Synthetiques de Géologie Expérimentale," p. 155. Paris, 1879.

SiO_2	....	68·4
CaO	....	12·0
MgO	....	0·5
Na_2O	....	14·7
Al_2O_3	....	4·9
		100·5

while after experiment it contained

SiO_2	....	64·5
CaO	....	21·9
MgO	....	1·2
Na_2O	....	6·3
Al_2O_3	....	1·4
H_2O	....	4·2
		99·5

The observations recorded in the present paper refer to structures developed in specimens of glass which, for the most part, were prepared, and all devitrified, at the Glass Works of Messrs. Pilkington Bros., St. Helens, and it is possible that they may have a special interest for those who are studying the natural devitrification of obsidians or other glassy or glass-bearing rocks, since in the cases here recorded the precise conditions of devitrification are known, while in the natural process we do not know the precise conditions. We may, however, assume with considerable safety, that within certain limits there will be a more or less close analogy between the results of the natural and artificial devitrification, allowing of course a margin for certain natural conditions which it would be difficult if not impossible to reproduce experimentally. Thinking that some definite laws might be arrived at by devitrifying solids of various forms, we have operated upon cubes, hexagonal prisms, trigonal prisms, spheres, cylinders, flat plates, and other distinct forms, and these we shall first describe. The difficulties attendant upon the microscopic examination of such materials consist principally in their excessive opacity in some cases, and in others upon the readiness with which the substances disintegrate during the process of grinding. The latter difficulty has in many instances been successfully overcome by Mr. Cuttell, by whom most of the sections have been carefully and admirably cut, so that the boundaries of the solids are preserved without injury.

Specimen No. 115 originally formed part of a 1-inch-thick piece of plate-glass, which was accidentally coloured green, during fusion, by

a small quantity of ferruginous material. The plate, measuring about 6 feet by 3 feet 8 inches, was devitrified in the following manner. A layer of sea sand, previously washed, sifted, and dried, was spread to a depth of $2\frac{1}{2}$ inches on the floor of a kiln used for annealing glass-house pots. On this sand the plate was carefully bedded and covered with more sand to a depth of 6 inches, the whole being kept together by a low brick wall. The object of bedding in sand was to prevent the bending and fusion of the plate when the kiln attained its highest temperature. The kiln was then lighted, and the heat slowly raised. In six days it had attained a dull red, and in six days more was at its full heat, a very bright red, sufficient to soften, but not enough to melt, the glass through its covering of sand. This temperature was maintained for twelve days, when the kiln was let out, and quickly cooled by opening the door. The mass of sand, however, retained its heat for a considerable time after it was possible to enter the kiln, for, on removing the dwarf retaining wall four days after the door was opened, the plate broke in consequence of cold air coming in contact with one of the edges, whilst the other parts were at a comparatively high temperature. The glass was thus in the kiln altogether for twenty-eight days, during six of which it was gradually heated to dull redness, during six more the temperature was increased to bright red, maintained at this for twelve days, and cooled during four. It was found to be thoroughly devitrified, and large pieces were ground with sand and water to a fine smooth surface on both sides, by which the thickness was reduced to seven-eighths of an inch. The portion selected for microscopical examination was broken off a corner of one of the large pieces. It is opaque, dull, and porcellanous externally on the ground surfaces, in places which have not been ground the surface is rougher, and has a glazed appearance. The parallel faces are of a pale green colour with a reticulation of white lines, enclosing areas which range from about $\frac{1}{32}$ to $\frac{1}{4}$ inch in diameter, mostly polygonal in form. The specimen was of irregular triangular shape, and the sides and edges formed by cracks, probably produced at an early period of the heating process, are of a uniform greenish-white or pale greenish-grey tint. The hardness appears to be slightly greater than that of the same glass before devitrification, upon which it produces feeble scratches. A cross fracture, revealing the devitrified interior, shows very delicate, slightly undulating bands, which agree in direction with the parallel faces of the plate, and the alternate bands, when viewed in this direction, exhibit a silky lustre like that of chrysotile, satin-spar, and other fibrous substances, only rather feebly. At the marginal extremities of this fractured surface similar bands are seen running in a direction at right angles to the others, and these transverse bands occupy two triangular areas, as shown in fig. 1.

Fig. 1.

In order to ascertain more precisely the nature of this structure, three sections were cut:—

A, at right angles to the parallel faces of the plate.

B, parallel to one of the parallel faces of the plate, and including between the planes of section little more than the greenish superficial layer with the white reticulations.*

C, parallel to one of the short sides, passing through one of the triangular areas near one of the angles, so that the section includes both sets of bands, cutting one set transversely and lying parallel to the other. These sections exhibit the general structure admirably when held obliquely in different directions between the eye and the light.

The following is a description of their microscopic characters:—

Specimen No. 115, Section A. When viewed by ordinary transmitted light under a low power, the banding already described is indicated by transparent or very translucent belts, alternating with bands of very feeble translucency. Both kinds of belts are traversed by fine lines, indicating a fibrous crystalline structure, which commonly shows a radiate arrangement, the divergent groups of fibres emanating from centres situated on or about the margin or edges of the section.† Each radiating group has what may be termed its own allotment, bounded by well-defined straight lines. The boundary of one side of an allotment sometimes consists of a single straight line, at others of two or more straight lines, meeting in very obtuse angles. The boundary lines are not curved. When the section is rotated between crossed Nicols, these allotments form a well-marked and important feature. The divisional lines in this section may be separated into two groups. The first group consists of five lines, viz., a median line, running parallel to the two parallel faces of the plate-glass, and four lines which form a bifurcation at either end of this median line, and enclose the terminal triangular areas. The second group of lines consists of those boundaries of the crystalline allot-

* This layer is not the original surface, $\frac{1}{16}$ of an inch having since been ground off.

† It must, however, be noted that the margin of the section lies about $\frac{1}{16}$ of an inch from the original surface, the $\frac{1}{16}$ of an inch hav ng been removed by grinding.

ments which run approximately at right angles to the surfaces which constitute the boundaries of the devitrified specimen. In polarised light the general aspect of the section is peculiar, and strikingly resembles a patchwork rug made of the skins of tabby cats. Further on we shall endeavour to account for this brindled appearance, which is represented in fig. 1, Plate I, as seen between crossed Nicols. The vertical edge of the section seen on the right of the field is the trace of one of the parallel faces of the devitrified plate-glass. The N.W. portion represents part of one of the terminal triangular areas, while the remainder shows some of the other crystalline allotments.

Specimen No. 115, Section B. This is a particularly interesting section. It is in fact one of the green surfaces of the devitrified plate-glass, *i.e.*, present surface, $\frac{1}{16}$ inch of glass having been ground away, and we can easily trace in it the polygonal structure already alluded to. Between crossed Nicols the polygonal areas are sharply defined and are irregularly clouded with crystalline aggregates, which appear dark. On rotating the section through 90° these dark aggregates become light, while the previously light portions become dark; we are, in fact, looking on the ends of bundles of crystalline rods. These polygonal areas are the cross sections of fasiculi of divergent crystals, and the boundaries of these polygons are shrinkage cracks, giving rise to a columnar structure, while the columns, like those of basaltic lavas, have their longest axes normal to the cooling surface. Fig. 2, Plate 1, shows the general appearance of this section, magnified eighteen linear, between crossed Nicols. As it seemed possible that greater amplification might afford more information concerning the nature of the little crystals which constitute these bundles, a $\frac{1}{4}$-inch objective was used, with the result shown in fig. 3, Plate 1. Only dark hazily-defined spots could be discerned between crossed Nicols, which became light on revolution of the section, while previously light portions became dark. The section has, in fact, the appearance of what is known as crypto-crystalline structure, and resembles, to a certain extent, some of the felstones, which, from other evidence, are known to have been once vitreous lavas. An examination of this section proves then that the polygons are the cross sections of the crystalline allotments of Section A, and that those allotments are longitudinal sections of polygonal, often pentagonal, prisms. Whether or not the polygonal jointing is connected with the crystalline developments, which it sheaths and separates, is a matter open to discussion. The Section B, when held between the eye and candle flame, presents the illusive appearance of being studded with concavities or convexities, from which it, we think, may be inferred that the radiate arrangement of the crystalline fasiculi originates at or about the centre of each polygonal area on the original surface of

the thick plate-glass.* If so it is possible that the strain consequent on crystallisation may have produced the prismatic fission. Fig. 1, Plate 3, might then be taken to represent portion of the surface of the slab at the commencement of devitrification, the dots indicating primary centres of crystallisation, while fig. 2 on the same plate would represent the development of prismatic structure by the formation of cracks between and around these centres of crystallisation. Fig. 3, Plate 3, shows one side (the lower one) of the block, fig. 2 the arrow denoting the direction in which the crystallisation advances. Apart from any hypothesis concerning the possible relation of the prismatic structure to the crystallisation, which may or may not be true, since it is possible that the prismatic structure was developed first, it is evident from the inspection of such a diagram that we may have a section giving prisms of very different widths, the width in section not necessarily representing the actual width of the prism, while in such a case the centre and general axis of the crystalline bundle may appear to be thrown on one side of the prism.

Specimen 115, Section C. This section truncates one of the terminal triangular wedges, of which mention has already been made, so that here we know for a certainty that we are looking on the cross section of the crystalline fasciculi belonging to the triangular area, and here we meet with precisely the same phenomena as those described and figured for Section B. On either hand the adjacent crystalline bundles emanating from the upper and under surfaces of the thick plate are seen lying in the plane of section, *i.e.*, we are looking at longitudinal sections of those bundles. In these we again see the cat-like brindlings. On the broken and partly ground away edges of this part of the section, a power over 500 linear shows that the crystalline bundles are made up of small fibres or microliths, closely packed side by side. The section is in all parts traversed by long, fusiform, or acicular brownish microliths, which lie with their longest axes in various directions, but usually across the general directions of crystallisation.

The brindled appearance in the crystalline bundles of these sections suggests at first sight the idea of pauses in the crystallisation, but when we find that by ordinary illumination the light is very faintly transmitted along these lines, some further explanation seems needful, and it seems probable that in these diverging crystallisations there is a kind of cone-in-cone or divergent composite structure, such as in that met with in the kidney-ore variety of hematite, or in clay-ironstone, the apices of the cones giving rise to a turbidity and being ranged so as to form successive arcs of approximately concentric circles, as indicated in fig. 6, Plate 3. From the evidence afforded by

* The surface of this specimen is $\frac{1}{16}$ of an inch from the original surface, which has been removed by grinding.

the sections now described, it seems certain that devitrification has in this instance commenced at the surface, and has proceeded inwards in directions at right angles to the different surfaces. Owing to its uniform rate of progression, the different sets of crystalline fasciculi have met along lines which divide the devitrified mass in a remarkably symmetrical manner, as shown in fig. 5, Plate 3, which represents one corner of the slab. That unequal rate of progression would cause a deviation from this symmetry is shown diagrammatically in fig. 4, Plate 3, and actually in the deflection of the diagonal line in fig. 1, Plate 1, Section A.*

Specimen G is a plate of flashed glass, about 2½ mm. in thickness, which has been completely devitrified under conditions similar to those described for Specimen 115, that is to say, it was imbedded in silver sand (previously washed and dried), placed in a kiln, and the temperature gradually increased during a period of eleven days up to a bright red. This heat was maintained pretty steadily for eleven days more, after which the kiln was quickly cooled, and the glass withdrawn. The flashed surface is of a deep blue colour, and is incrusted with grains of sand. The opposite face is mottled with small dull green and greenish-white spots, and has a surface like coarse glazed pottery. Flashed glass was chosen in this case, as it was thought possible that some of the pigment might be carried inwards by the crystallisation. This, however, does not seem to have taken place to any great extent, for on examining a thin section taken at right angles to the broad surface of the plate under a power of 250 linear, the blue layer is seen to have remained on the surface, although its boundary is ill defined, and the bluish tint extends for only a very little distance inwards, gradually fading away. On the outer surface of the coloured layer there has, however, been a considerable disturbance of the blue glass, which appears to have been fused, and to have run between the sand grains against which it was bedded, *ff*, Plate 2, fig. 2, forming a cement of blue crystalline sheaves. The crystalline structure of the blue layer is throughout very irregular, consisting of similar sheaf-like aggregates and interlacing crystals. Passing from this layer we find the contiguous glass converted into radiating crystalline groups, separated by sharply defined joint planes,

* *Supplementary Note.*—Specimen 115. Thermal conductivity appears to be uniform on the large parallel faces of the plate, both at the margin and at a distance from the margin. The isothermal curves are also circles on the sides of the plate at right angles to the large faces. On a transverse section of the plate which traverses the crystalline fasciculi in directions both longitudinal and transverse, as in Section C, the wax also melts in circles both on the area of the longitudinal and on that of the transverse sections. This accords with the statement of M. Ed. Jannettaz ("Propagation de la Chaleur," "Bull. Soc. Géol. de France," 3e Serie, t. ix, p. 200) that minerals having a fibrous or lamellar character do not conduct heat better in the direction of the fibres or of the lamellæ than if they had no such structure.

ij. Plate 2, fig. 2, which traverse the plate normal to the large parallel surfaces. These joints are evidently the boundaries of polygonal prisms, and it is the ends of these prisms which cause the green and white spotted appearance on one surface of the specimen, while the reason that no such marking is visible on the other surface is partly due to the screen of sand grains which covers it, while beneath there would be no such markings until we reached the layer of originally white glass, because the joints do not appear to traverse the irregularly crystalline blue layer. Divergent crystallisations, also bounded by prismatic joints, start from the green spotted surface of the plate, and the two sets of divergent crystallisations meet in an undulating line, *ll*., Plate 2, fig. 2, which approximately divides the plate into two plates of about equal thickness. The joint planes on the opposite sides of this line do not coincide, and the halves of the plate if separated along the surface, of which this undulating line is the trace, would doubtless present a mammillated appearance. The general structure reminds one of that of part of a much flattened chalcedonic geode. It will be seen that in this specimen the devitrification has taken place on precisely the same principle as in the thick plate previously described. There has been a prismatic structure developed normal to the bounding surfaces, divergent crystallisation occurs within the prisms, and these crystalline fasciculi advanced in opposite directions until they arrested one another, but the line of arrest in this case is sinuous, while in the preceding specimen the lines of arrest are straight. On examining the section under a power of 50 diameters, fine lines, like small scratches, are seen to cut across the divergent crystallisation. Under much higher powers they appear as rod-like microliths, and they lie with their longest axes in all directions, but mostly transverse to the divergent fibres.

Specimen I is part of a completely devitrified square prism of plate-glass. The devitrification of this specimen was brought about by two separate operations. The whole of the prism, about 4 inches in length, was bedded in silver sand and heated during four days to a temperature gradually increasing from that of the atmosphere up to a red heat, maintained at that for two days more, and then quickly cooled. When cold it was broken in two and found to be regularly devitrified to a depth of about 1½ mm., the interior being unaltered. One of the halves was then burnt again, this time for the same period and under exactly the same conditions as Specimen G, *i.e.*, bedded in sand, brought gradually to a bright red, maintained steadily at that heat for eleven days, and then quickly cooled. The faces are of a pale greenish-yellow, have a glazed appearance like that of pottery, and are traversed by a network of very fine cracks. When the specimen is held before a strong light these surfaces present a spotted appearance, similar to that seen on the plane surface of other devitrified

solids. Under the microscope it is seen that the crystallisation has advanced as usual from the surfaces inwards. After passing through a distance of about $1\frac{1}{2}$ mm. from the surface there has been a pause, marked by a fairly well-defined line, indicating the extent of the devitrification produced by the first heating to a red heat. This line is not straight, but has a series of slight convexities directed inwards, each convexity being bounded by joint planes normal to the surface. The prism therefore had first of all a devitrified envelope, the inner surface of which was mammillated, and each mammillation was the termination of a small prism. As the crystallisation advanced from the inner surface of this envelope, a fresh series of less numerous joints was developed, giving rise to a coarser prismatic structure, and between these joints we see in section a beautiful divergent crystallisation, each divergent group originating on the inner surface of the first crystalline envelope, a single prism sometimes containing only one such group, at others several. The general direction of these prisms is normal to the surfaces of the devitrified specimen, and the lines of arrest would join the opposite angles of the square section, were it not that in this particular slice an irregular pentagonal area occurs, against four of whose angles the lines of arrest abut. This irregular pentagon is a transverse section of another set of divergent crystallisations, whose longest axes would diverge from the axis of vision, and they evidently emanated from one of the basal planes of the large devitrified square prism, or from a transverse fracture as the prism was broken across after the first heating. Had the specimen been a cube, a section taken parallel to two of its faces and passing accurately through the centre of the cube, would merely have shown two continuous lines of arrest joining opposite angles and intersecting in the centre of the square section, assuming, of course, that the crystallisation advanced equally from all six faces. Such a structure would divide the cube into six equal four-sided pyramids, as indicated in the diagram, Plate 3, fig. 7. In the specimen before us the crystallisation has advanced rather irregularly, and the lines of arrest are consequently not continuous straight lines, but continuous series of straight lines, a repetition, in fact, of the conditions indicated in the diagram, fig. 4, Plate 3.

Specimen H is portion of a similar square prism of plate-glass, heated gradually for six days to a red heat under exactly the same conditions as the first operation on Specimen I. It differs from the preceding specimen in having been devitrified for only a slight distance from the surface. A section of the crust through one of the angles presents an appearance precisely similar in character to that of the crust of Section E_2 (a six-sided prism), figured on Plate 2. These are groups of divergent crystals which pass from the surface inwards, and are separated by prismatic jointing. The inner surfaces of each

crystalline group is convex, the convexity being directed towards the interior of the solid. When magnified between 500 and 600 diameters these convex surfaces are seen to be fringed by the projecting terminations of the divergent crystalline fibres.

Specimen K. This is part of a completely devitrified trigonal prism of plate-glass, devitrified by two operations, under precisely the same conditions as Specimen I, and the section has been taken parallel to the basal plane. The general principle of devitrification elucidated by the examination of the preceding specimens may also be clearly recognised in this case, but the crystallisation, after the first envelope was formed, advanced in a somewhat irregular manner, which needs interpretation. The irregularity in the crystallisation of this specimen may be attributed to the fact that there is a flaw in it. The general structure is shown in Plate 3, fig. 8. Here we notice first of all the envelope or devitrified crust, due to the first heating operation, in which there is prismatic structure and a series of divergent crystallisations trending inwards. Next comes a similar but coarser series of prisms also normal, or approximately normal, to the sides of the trigonal prism, and in these the divergent crystallisation has also travelled from without inwards. So far there is no deviation from the general principles of devitrification which we met with in the preceding specimens, in fact the crystallisation has proceeded inwards as usual, in directions approximately normal to the limiting planes of the devitrified solid. We now meet, however, with an apparent exception to the general rule, for the three sets of crystalline fasciculi, instead of continuing their course until they arrest one another in three straight lines joining the angles and the centre of the triangular section, are suddenly arrested and enclose an area rudely shaped like a three-rayed star, this being subdivided into three irregular portions. The deltoidal areas are traversed by cracks, and from points along these lines we have groups of crystals diverging on *both sides* of the lines. They have consequently travelled from within *outwards*.

The different areas of devitrification are by no means symmetrically disposed. Diagram, fig. 8, Plate 3, shows, with approximate truth, how the parts of the actual section really occur. It will be seen on reference to this figure, that at the point *a* there is a crack which extends in a curve towards *b*. From a point on the curved line *ab*, about opposite to the middle of the edge in which the crack *a* occurs, another nearly straight crack passes to *c*, and from the inner surface of the devitrified crust a third crack extends in a curve from the little fissure *a* to the point *d*. The crystallisations diverge on both sides of these three cracks. They are bounded by prismatic joints, which are continuous across the cracks, and each pair of crystalline fasciculi diverges from a common centre situate on the crack and between a pair of prismatic joints. These three distinct areas of crystallisation

are very irregular in form, and this has already been attributed to the presence of the flaws emanating directly and indirectly from the fissure. There appeared to be no reason why in such a solid the devitrification should not proceed steadily inwards until the three sets of prisms arrested one another along three lines passing from the three angles of the triangular section, and meeting in its centre.

With a view to settling this point another trigonal prism (Specimen No. 143), free from any flaws, was devitrified. A transverse fracture through this devitrified prism shows three distinct and similar areas of crystallisation; each is an isosceles triangle. These triangular areas are bounded by the three sides of the prism and by three straight lines of arrest, which accurately join the centre or axis of the prism with its three angles or edges, fig. 9, Plate 3. This demonstrates conclusively that the irregular devitrification seen in Section K is due to simultaneous crystallisation along flaws.

Specimen D heated twice under same conditions as Specimens I and K, is part of a completely devitrified six-sided prism of plate-glass. The surface has a glaze like that of pottery. The transverse section of the prism is not a perfect hexagon, and it has not been cut quite at right angles to the principal axis. There is a well-marked crust of divergent crystalline fasciculi due to the first short heating, prismatic joints being also present, but they are not well defined. Devitrification has then proceeded inwards in directions approximately normal to the lateral faces of the prism in broad divergent crystalline groups, separated by joint planes, which preserve at the best a very imperfect parallelism. In fact the prismatic structure which they indicate seems very irregular, and in the section a prism is often represented by a lanceolate or an irregularly shaped area, while the divergent crystallisations do not all seem to be formed in directions parallel to the plane of section. These crystallisations show brindled markings, similar to those seen in Specimen No. 115. There is strong chromatic depolarisation in this, as also in the preceding Sections I and K.

Specimen E is part of a six-sided prism of plate-glass, 2 cm. in diameter, which has been devitrified to a depth of barely $1\frac{1}{2}$ mm. under precisely the same conditions as Specimen H. The devitrified crust is yellowish-white, and has a glazed surface like that of pottery. Two sections have been cut from this specimen, E being taken transversely to the principal axis, and E_1 parallel to it and to one of the faces of the prism. The latter section consists, in fact, only of the devitrified crust of one of the faces of the prism.

Section E, taken transversely to the principal axis of the six-sided prism, shows a devitrified crust, which by reflected light looks white, while by transmitted light it appears under the microscope of a brown or yellowish-brown tint. It consists of divergent groups of very delicate acicular crystals, but even under high powers their termina-

tions, where they shoot into the unaltered glass, cannot be clearly made out. In most cases their terminations appear to be rounded, while in others they have a rectangular aspect, suggestive of a basal plane or an edge normal to the principal axis. The groups are not separated by prismatic joint planes, but the divergent crystals of adjacent groups appear to slightly overlap. The directions of extinction indicate that they are possibly rhombic forms. When magnified about 570 diameters the individual crystals seem frequently to consist of linear aggregates of minute globulites, but this appearance is possibly deceptive, and in some cases the crystals exhibit no such structure. The terminations of the crystals pass rather irregularly into the adjacent glass, giving the edges of the crystalline groups a fringed aspect somewhat like the pile of velvet. The adjacent glass shows colourless spheroidal specks or globulites.

Section E_2 is taken parallel to one of the faces of the six-sided prism, and is, indeed, a shaving of the devitrified crust. Mr. Cuttell succeeded in making a section the full size of the face, and from this the drawing, fig. 1, Plate 2, was made. In this drawing a basal and lateral edge are shown, and it will be seen that from these edges divergent groups of crystals pass inwards. With the exception of this fringe, which represents more or less oblique sections of the crystalline groups which constitute the devitrified crust, the remainder of the face shows only a polygonal network, the polygons being the cross sections of prisms. It has, in fact, the same structure as the margin, only the crystalline groups are in this part cut transversely to the direction of their growth, while at the margin they are cut obliquely, for the section being taken a little distance inwards from the surface of the face trenches slightly upon the crystalline groups of the adjacent faces, both lateral and basal. The section as originally cut was so feebly translucent that an endeavour was made to reduce its thickness. This, however, resulted in its almost total disintegration along irregular cracks without materially increasing its translucency.

Specimen F. This is a completely devitrified sphere of light-coloured bottle-glass 18 mm. in diameter, devitrified in two operations under the same conditions as Specimens I, K, and D. Under the microscope a section taken through the centre of the sphere shows a somewhat irregular circumference, which is accounted for when the surface of the original specimen is carefully examined, for it is seen to be pitted with numerous small cavities, and to have a rough fritted and imperfectly glazed aspect. The irregularities of this surface are due to the impressions of sand-grains, a few of which may still be detected adhering to the surface. The glass has evidently undergone incipient fusion, and the crystallisation in the immediate neighbourhood of the sand-grains is very small and confused. This irregularly

crystalline margin is bounded internally by a sinuous crack, showing the extent of the devitrification produced by the first heating to which the specimen was subjected, while other irregular cracks traverse this portion circumferentially as a rule, but they sometimes pass through the margin radially. The latter are few, penetrate but a short distance, and are mostly fringed by delicate crystalline fibres normal to the crack, and usually terminate in a radial group of fine acicular crystals or fibres. In one or two spots the cracks are seen to follow the contours of small cavities, from which sand-grains have been stripped in the process of grinding the section. Inside the wavy circumferential crack the crystallisations have shot inwards in long divergent groups, which towards the middle portion of the sphere give place to large irregular radiating groups of crystals, so large, in fact, that there does not appear to be more than half-a-dozen of them in the section, and these are in most instances cut through in a plane remote from their centres, thus giving oblique and transverse slices through the crystalline rods. Had these groups been able to crystallise freely they would have resulted in spherules, and this, indeed, might have been the case had the devitrification of the sphere been incomplete; as it is, they seem to have rudely polygonal boundaries. The devitrification of this specimen seems in part to be of a micro-crystalline-granular character under a magnifying power of 18 linear, but under a power giving an amplification of 570 diameters this is seen not to be the case, the mass being resolved into a closely matted aggregate of little acicular crystals with a general tendency to radiate grouping, as shown in fig. 4, Plate 1. In fig. 5 on the same plate the general aspect of a portion of the sphere at and near the margin is shown. The circular hole near the margin is where a sand-grain, around which the glass has fused, has been stripped out in grinding.

Specimen No. 78 is portion of a large hemispherical mass of completely devitrified sheet-glass taken from a mass of many tons which burst from the furnace in the liquid state and ran into a "cave" underneath. The mass solidified rapidly, but owing to its great bulk remained at a high temperature for several days. In the specimen there is a fragment of uncombined lime, indicating that at the spot from which the specimen was taken the fusion of the raw materials composing the glass was not quite complete. This specimen exhibits a curious and very rough concentric scaly or platy structure. It is of a pale greenish-white tint, and the broken surfaces are covered with small glistening hair-like crystals. It feels rough to the touch like a piece of unglazed porcelain, which it rather resembles, and it has a distinctly vesicular structure. The vesicles are spherical. In thin section it is very feebly translucent, and consists of a mat of minute groups of radiating crystals. The aspect of the surface of a

roughly broken piece of this specimen magnified four diameters is shown in fig. 5, Plate 2.

Specimen No. 105 is a piece of plate-glass 12 mm. in thickness, having the uneven surface usual in plate-glass before it has undergone the process of grinding. Devitrification in this case has given to the glass the appearance technically known as "burnt," and it was brought about in the ordinary process of annealing owing to the kiln being too hot. The glass was in the stiff, pasty condition suitable for rolling when introduced into the kiln, and was kept at a bright red by flame playing almost directly upon it for about half an hour, during which, and possibly during a short period of subsequent cooling, the devitrification was effected. The devitrification of this specimen is quite incipient, and affects merely the two parallel surfaces, one of which is uneven and scratched owing to contact when in a soft condition with the rough bed of the kiln. This latter surface is extremely interesting, as it shows a reticulating series of irregular cracks, traversed in places by straight belts of spherules which are apparently in no way connected with the cracks, and begin and end abruptly in a seemingly capricious manner, fig. 4, Plate 2.* The cracks are similar to those produced in glass by heating it and plunging it in water. Other isolated and larger spherules are also to be seen upon both surfaces of this specimen. Fig. 3, Plate 2, shows one of these surfaces—the upper, as seen by ordinary transmitted light, and magnified 32 linear. The shaded spots represent incipient spherules which fail to show any depolarisation; the darker spots are decided spherules with strong depolarising power. The unshaded portion of the section also transmits light between crossed Nicols, and is therefore in a state of strain. Under a magnifying power of 1150 diameters the incipient spherules can merely be resolved into brownish granular patches, sometimes approximately round, not uncommonly dumbbell-shaped, or like two coalescing spheres, but usually they are of irregular form, and their general aspect is nebulous.

Specimen L. A piece of pale greenish sheet-glass transferred, when in the semi-fluid state suitable for working, to a small pot in which it was maintained during four or five hours at a temperature barely sufficient to permit of its being "gathered." It is traversed by rudely parallel, irregular, flocculent, milky bands. Under a power of about 250 diameters numbers of minute crystallites are visible; they show no double refraction. Some are stellate, others fusiform or acicular. The latter are often wholly or partially surrounded by fine dusty segregations, which frequently seem to be diminutive divergent spicules. The most common forms have the aspect of monoclinic or

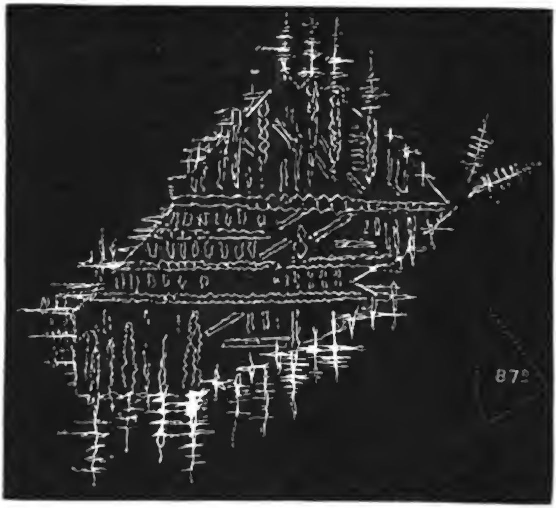
* This specimen closely resembles some of the spherulitic obsidians of Montana, U.S. Compare this figure with fig. 5, Plate XX, "Quart. Journ. Geol. Soc.," vol. xxxvii.

triclinic crystals. Their angles vary considerably; one gave 150°, another 116°, but these measurements are of little value, as it is doubtful whether the individuals measured were lying parallel with the planes of section. Some of these forms are shown in fig. 6, Plate 2, not represented as they are actually grouped in the preparation, but selected from various spots. They closely resemble some of the crystallites met with in the slags of blast furnaces. They occur in the white turbid bands in the glass, the transparent portion being almost free from them.

Specimen M is a piece of clear sheet-glass, about 2·5 mm. thick, from a pot containing somewhat less lime than usual. Owing to the furnace being rather cold during the time the glass from this pot was being worked, devitrification in the form technically known as "ambitty" set in, and increased to such an extent that blowing was stopped and the pot emptied by ladling. The specimen was blown shortly before the ladling operation was commenced; it contains a few very beautiful crystallites similar to those figured in Plate 8 of Vogelsang's "Krystalliten." One of them, which closely resembles one of the usual forms of snow-crystals, being a skeleton hexagon or six-rayed star, gives angles of 60° between the component crystalline needles. These exhibit double refraction, and undergo extinction in directions parallel to and at right angles to their longest axis. Between crossed Nicols depolarisation from strain is visible in the adjacent glass, the minute brushes of light being more intense about the points of the principal needles. It would appear from the depolarisation and directions of extinction that this crystallite may be referred to the rhombic system, twinned somewhat after the manner of chrysoberyl. This seems the more probable, since some of the forms in Section L also resemble certain rhombic forms. The crystallite just described is seen when examined under a power of about 280 linear to be traversed by an irregular network of strong cracks lying in the same plane as the crystallite, and extending nearly to but never beyond its margin. In the centre of the crystallite is a dark spherule. The fact that the reticulating cracks are restricted to the area occupied by the crystallite indicates a relation to the latter, and the depolarisation of the adjacent glass indicates strain. Since this strain-depolarisation only occurs at the margin of the crystallite, we may infer that the strain is connected with its development, and the cracks are no doubt the result of this strain. Had the body been a completely developed crystal and not a skeleton form, the strain would probably have resulted in the development of a perlitic crack, and not in a reticulating series of cracks which possibly arise from strain about a number of points. Another crystallite in the same piece of glass is very different in appearance to that last described; its general outline is that of an irregular hexagon. It is traversed by four well-

marked crystalline rods, apparently composed of piles of octahedra like those of alum, and where they touch the margin of the crystal they usually pass beyond it, forming little spicular crystallisations like fir-trees or like the crystals formed in cast iron. They throw out branches at right angles to the main spicule. The crystallite is also traversed by other crystalline rods of a like character, but at right angles to the first set, and these also pass out in little fir-tree-like crystallisations. There are also small rods which run in two directions obliquely to the former, and which intersect in an angle of about 87°. The form therefore is not cubic, as might at first sight be thought. The larger spiculæ also show double refraction. There is some depolarisation in the glass around this crystallite due to strain, but no cracks are developed. The spiculæ extinguish parallel to and at right angles to their longest axes. At least they appear to do so, but it is difficult to tell, and the colour difference is so slight when a Klein's plate is employed that it is impossible to speak with any certainty on this point. On the whole we are inclined to regard these crystallites as belonging to the rhombic system. The one last described is a twinned form, and exhibits several re-entering angles. A rough sketch of it (fig. 2) is appended.

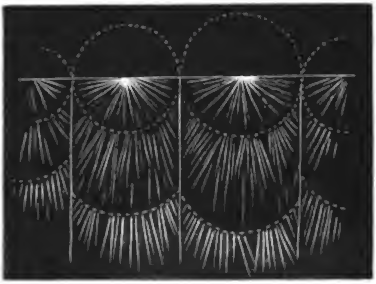
Fig. 2.



Generalisations.

From the microscopic examination of the specimens already described,* it seems evident that the devitrification of solids of the nature described in this paper takes place in a definite and apparently uniform manner, to which Specimen No. 105 is no exception, for the incipient spherules and the well-developed spherules are but rudimentary phases of the divergent groups which we generally meet with, and which have been already described. In Specimen 105 they are essentially superficial, and we can imagine them as hemispheres, as represented in fig. 3, ready, as devitrification advances, to be continued inwards, in which case we cease to recognise their spherulitic

FIG. 3.



character. In solids free from flaws the devitrification appears then, as a rule, to consist in the development of divergent groups of crystals, the divergence being usually limited by a network of minute joints, which give rise to small polygonal prisms. These crystals and joints extend inwards from the different faces of the solid, and may or may not ultimately meet. The crystalline groups in their respective prisms are banded by arcs of circles, which we may assume are related, but perhaps obscurely, to the initial pseudo-spherulitic structure of the superficial crust of the solid. These arcs indicate successive stages of growth. The crystallisations from the different faces of the solid ultimately, in small masses, arrest one another, and devitrification is then complete. In the case of the sphere, Specimen F, already described, the process has gone on in much the same

* With the exceptions of Specimens 78, L and M, in which devitrification was produced during cooling from the fluid state, and Specimen 105, which was probably still somewhat soft when devitrification commenced, all the specimens described were devitrified whilst in the solid state by more or less prolonged periods of heating.

manner for a slight distance from the surface, after which an irregular crystallisation has been set up from independent centres; but it should be remarked that difference in the chemical composition is known to influence the mode of procedure, as well as the character of the devitrification. The direction of the prismatic structure always seems to be approximately normal to the surfaces, and the divergent sheaves of crystals advance from the surface inwards by successive growths within the prisms. It seems quite possible that in the absence of such prismatic jointing the whole mass would become spherulitic, or would consist of an irregular felted mass of crystallites. The near resemblance which some of the specimens just described bear to devitrified and partially devitrified obsidians shows how close the structural relationship is, and that, allowing for difference in the conditions under which the process takes place, the principle of devitrification is the same.

Specimen No. 122*d*, a piece of ordinary sheet-glass, which was bedded in white sand and heated during a period of only four days to a temperature gradually increasing from that of the atmosphere up to a blood-red—a temperature somewhat lower than that employed for any of the specimens previously described, shows purely superficial devitrification by the development of globulites and spherules or spherulitoid crystallites, like fig. 11, Plate 3. In this particular crystallite, which is of a pale brown colour, no structure can be made out. It seems merely to consist of an aggregate of globulites, but in other cases bodies of precisely similar form show a decided radiating crystalline structure, like that of the brown spherules, which occur with them in the same specimen, the only difference between these crystallites and the spherules consisting in the external form or limiting surface. It is for this reason that we propose to call them spherulitoid crystallites. Fig. 10, Plate 3, drawn from the same piece of glass, shows part of the network of cracks by which the surface is cut up, and the curious manner in which the globulites have segregated along these cracks, so as to leave the fairly well-defined circular and oval spaces in which the globulites are less densely packed. Spherules sometimes occur within these clearer areas, but the latter do not seem to have any necessary connexion with the development of the spherules.

In Specimen No. 122*b*, superficially devitrified under the same conditions as the preceding, a tendency to the formation of perlitic structure is seen around some of the spherules.

Specimen 126, a piece of rough plate-glass, $\frac{1}{2}$ inch thick, bedded in white sand, contained in a small fire-clay pot, and placed in a kiln, the temperature of which was gradually raised during a period of $8\frac{1}{2}$ days, by which time a dull red heat, about 650° C., was attained. As it was known by comparative experiments with similar pieces of

glass contained in other pots in the same kiln, that no appreciable change had taken place in the glass up to this time, we propose to reckon, in this and subsequent experiments, what may be called the *active* period of devitrification, from the first attainment of 650°, neglecting altogether the time required to bring the specimen up to this temperature, which necessarily varies in different cases, and is known to be without appreciable effect on the glass. The pot containing Specimen 126 was withdrawn from the kiln 29 hours after its first attaining the temperature of 650°, by which time the heat had slightly increased. The pot with its contents was allowed to cool during about four hours, when the glass was removed from its covering of sand, which had cooled down almost to the atmospheric temperature. This specimen shows devitrification only on the surfaces, the alteration being so slight that writing can be clearly read through the glass when it is placed close over it, but when raised an inch above the writing the latter appears blurred and illegible. The devitrification, which is quite incipient, consists in the segregation of vast numbers of minute granules and globulites about various points on the surface of the glass, and in very many cases small stellate crystallites lie in the midst of these segregations. They are colourless and translucent, but too small to show any double refraction, even if they possess the property. Under an amplification of 120 linear the specimen shows portions of its surface which are still quite clear and unaltered. The margins of the unaltered areas show some fine nebulous segregations which envelope no crystallites, but the majority contain the stellate forms already alluded to. Of these, the simplest form is a four-rayed star or cross, the arms of the cross being apparently at right angles, but most of these crystallites are many-rayed, as shown in fig. 13, Plate 3, which was drawn with a magnifying power of 820 linear. On the top and left hand margin of this drawing portions of a crack are shown, and on certain parts of the surface of this specimen the nebulous and crystallite-bearing spots are separated by a network of irregular cracks.

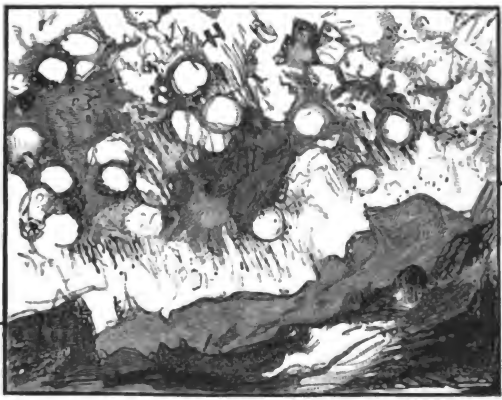
Specimen 127, a piece of polished plate-glass, $\frac{3}{4}$ inch thick, treated in the same pot as Specimen 126, and under exactly the same conditions. This is another instance of incipient and purely superficial devitrification. The general appearance is somewhat like that of No. 126, but in this case, although a few imperfectly developed spiculæ are present, there are no distinct stellate crystallites, possibly because in glass of this kind, containing a considerable quantity of lime, stellate crystallites do not occur so frequently as in the quality represented by Specimen 126, and the alteration of the glass consists merely of delicate nebulous spots, which under a power of 820 linear are seen to be composed wholly of globulites, and this is the most rudimentary phase of devitrification touched upon in this paper.

The little nebulous patches are mostly circular in form, and these circular patches often coalesce. There are a few instances in which the globulites occur within sharply defined circular or approximately circular boundaries, but for the most part the nebulous patches shade gradually away into the glass. One of these patches magnified 820 diameters is shown in fig. 12, Plate 3. The structure foreshadowed in this and in Specimen 126, may be regarded as spherulitic.*

Specimen No. 147 is especially interesting on account of the perfect manner in which it demonstrates that devitrification takes place from the surfaces of a crack, just as from any other surfaces. The

* A very interesting example of a like structure, but on a much larger scale, is seen in a specimen of obsidian collected by Mr. John Arthur Phillips, at Hot Springs, near Little Lake, in California. The obsidian is black and contains several greyish-white, or yellowish-white, spheroidal bodies (lithophysen of Richthofen), which range up to an inch in diameter. These, when examined carefully, are seen to consist of numbers of small spherules, about $\frac{1}{32}$ of an inch in diameter, but many of still smaller dimensions. The minute spherulitic structure of these large spherules is best seen on weathered surfaces, but even on fractured surfaces the spherules may still be seen, though their spherical character is less clearly visible, owing to interstitial matter, which becomes removed by weathering. In these larger spherules there is evidence, though obscure, of a radiating structure. The mimicry of the little spherules built of globulites, in Specimens 126 and 127, by these large spherules built of little spherules, in the obsidian, is very striking, but it is quite probable in the latter case that the smaller spherulitic structure was set up in the large spherule after its formation, the vestiges of a radiating crystalline structure tending to confirm this view.

FIG. 4.



Part of large spherule in obsidian from Hot Springs, near Little Lake, California.

Fig. 1

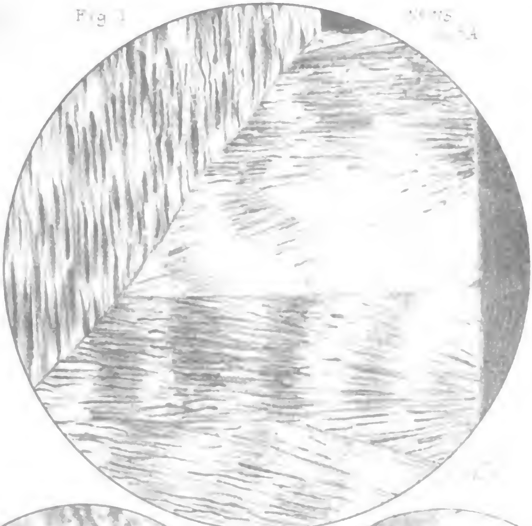
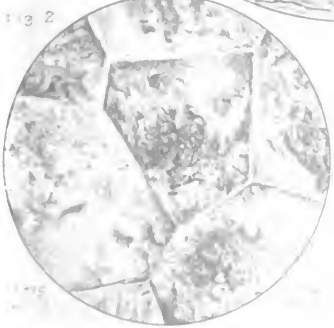
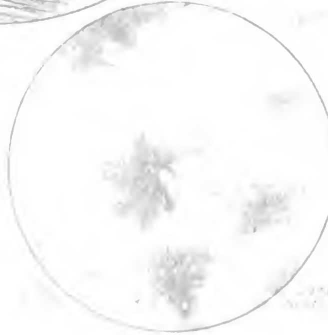
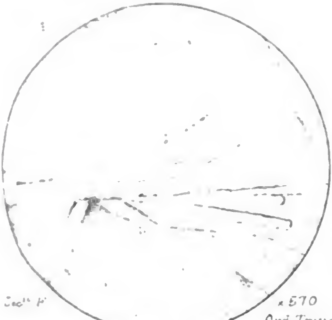


Fig. 2

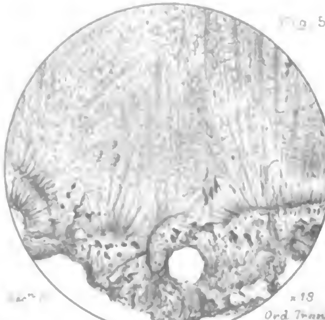






Sect. F x 570 Ord. Trans. Lt.

Fig. 5



x 18 Ord. Trans. Lt.

Fig. 1

Fig. 2

Fig. 3

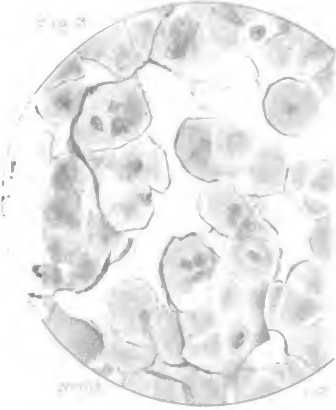


Fig. 4

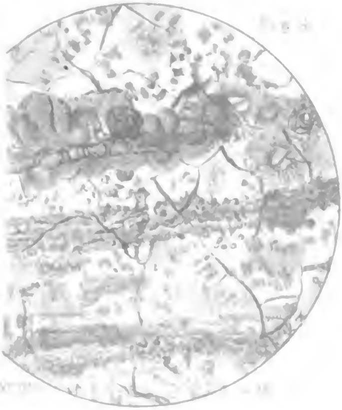


Fig. 5

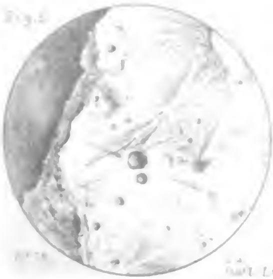
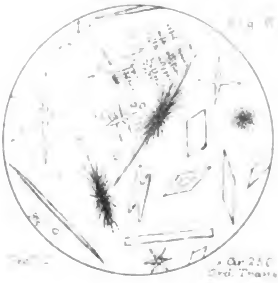


Fig. 6



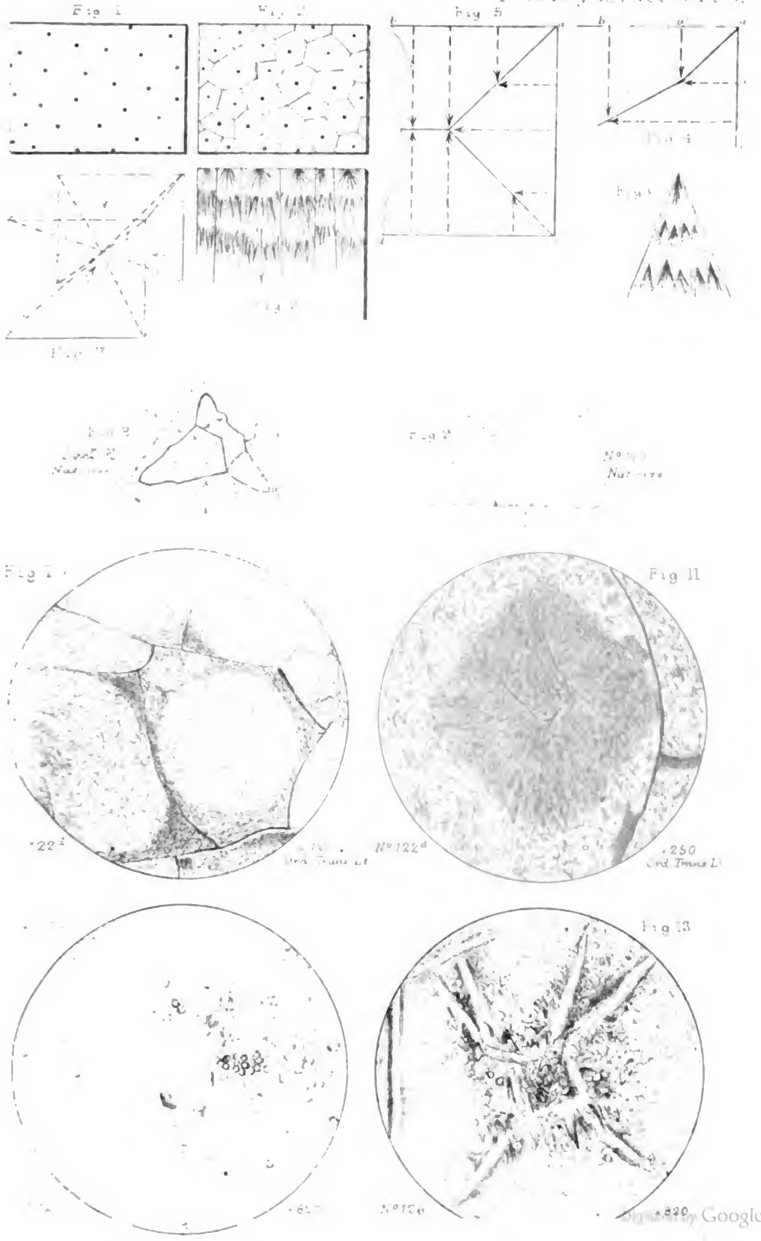


Fig 1
Fig 5
Fig 4
Nat. size
Fig 11
No 122d
×250
Ord. Trans. Lt
Fig 13
No 126
×820

Fig 1

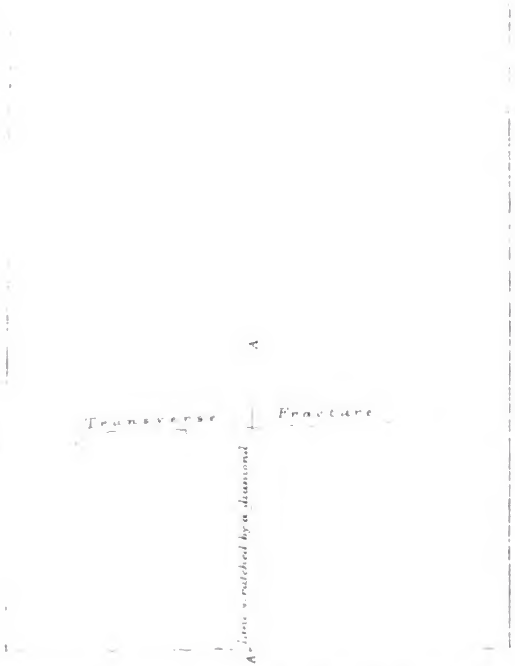
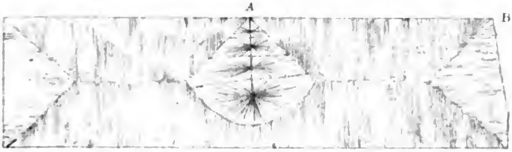
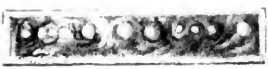


Fig 2



Fractured surface along the line BB

Fig 4



Nat size

Fig 3

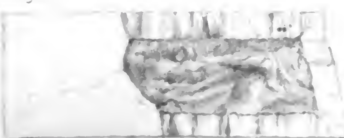


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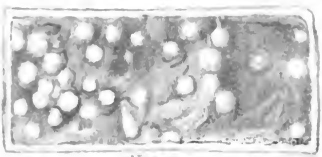
Fig 5



Nat size

specimen is a slab of $\frac{3}{4}$-inch British plate, about 4 inches by 3 inches in diameter, and upon one of its surfaces a straight cut or scratch, about 2 inches long, was made by a diamond, producing an exceedingly fine crack, extending at the edge of the plate to a depth of over $\frac{1}{2}$ inch in a direction approximately normal to the surface upon which the scratch was made, and gradually dying out to the end of the diamond cut. The crack was sufficiently fine to show Newton's rings. The specimen was then completely devitrified by heating continuously for nine days at a bright red heat, a temperature considerably higher than was employed in the case of Specimen 126, 127, and it was subsequently cracked across in the direction of the line marked BB in fig. 1, Plate 4. Fig. 2 represents the fractured surface. At each end are the usual triangular areas, formed by lines of arrest, but the line of arrest which usually joins the apices of these triangular areas is here interrupted by another series of crystallisations which have emanated from the crack produced by the diamond scratch. In the half of the plate nearest to the scratched surface we have, indeed, a reproduction of what has taken place at the outer edges of the plate, the result being a nearly equilateral triangular area of crystallisation, bisected by the crack already mentioned. This crack, however, passes a little beyond the median line of arrest, and from its termination the crystallisation radiates and ends against a curved arrest line, as shown in fig. 2, Plate 4.

That devitrification does not always proceed in the orderly and uniform manner seen in Specimens 115, 147, and, indeed, in nearly all of the examples already described in this paper, will be best realised by reference to the figures of Specimens Nos. 132 and 136 on Plate 4, figs. 4 and 5. Fig. 3 in the same plate, Specimen No. 137, is a small slab of glass partially devitrified. The crust has been formed in the usual way by crystallisation proceeding from the surface inwards, but the process has been arrested, and where the outer crust is broken away a core of somewhat cracked but perfectly clear glass is seen, in which no spherules or other crystalline bodies are visible. In Specimens 132 and 136, however, the result has been different, for after a slight external crust has been formed, devitrification has also started from numerous points within the glass, giving rise to a well-marked spherulitic structure.

Why these spherules have been formed instead of a gradually increasing crust is a matter which we hope to explain in a subsequent paper.

The Bakerian Lecture.—"On the Corona of the Sun." By William Huggins, D.C.L., LL.D., F.R.S. Received June 11, 1885. Read June 11, 1885.

Περὶ δὲ τον χρόνον, ὃν ἐν τῇ Ἑλλάδι ἐνεσπούδαζεν, ἐπεῖχε τὸν οὐρανον διοσημία τοιαύτη, τὸν τοῦ Ἡλίου κύκλον περιελθῶν στέφαρος, ἐοικὼς Ἴριδι, τὴν ἀκτῖνα ἠμαυροῦ.

Philostratus, "Life of Apollonius," bk. viii, ch. xxiii (ed. Leipzic, 1709).

Ἀλλὰ περιφαίνεταί τις αὐγὴ περὶ τὴν ἴτυν, αὐκ ἐῶσα βαθεῖαν γίνεσθαι τὴν σκιὰν καὶ ἄκρατον.

"Plut. Opera Mor. et Phil.," vol. ix, p. 682 (ed. Leipzic, 1778).

The sun is the only star the corona of which we have been able to observe, for all other stars are too distant to give true images in the telescope. If the sun were removed to a distance equal to that of the nearest star, its disk would subtend less than the one-hundredth of a second of arc. We have also to consider the small relative brightness of the corona, the light from which has been estimated at different times to be from $\frac{1}{100000}$ to about the $\frac{1}{400000}$ part of the sun's light. It is, indeed, possible that stars which have a higher temperature than our sun, are surrounded by coronæ of greater extent and brightness.

At the eclipse of 1882, some information was obtained of the sun's condition in relation to that of the brighter stars. The photographs of the more refrangible part of the spectra of stars, which I had the honour to lay before this Society in 1879,* gave a clue by which the stars could be arranged in a serial order, at the head of which stand the bright stars Vega and Sirius. I ventured to suggest that the differences in their spectra might be due primarily to temperature; and even to make the further suggestion, that the hotter stars were probably the younger stars, and that we had obtained possibly some indications of the relative ages of the stars. The position of the sun came some distance down in the series, very near the position of Capella, and just above the stars which begin to show a yellow tinge in their light. In the ordinary solar spectrum it is difficult to distinguish the ultra-violet group of hydrogen lines, upon the character of which this serial arrangement was mainly based, but in the photograph of the spectrum of the corona obtained during the Egyptian eclipse, Captain Abney and Professor Schuster have been able to recognise very thin bright lines corresponding to the lines of this group.† These lines were not due to the corona, but to

* "Proc. Roy. Soc.," vol. 30, p. 20; also "Phil. Trans.," vol. 171, p. 669.

† "Phil. Trans.," 1883, p. 267.

prominences at the base of the corona. The thin condition of these lines, as well as the breadth of the lines of calcium at H and K, confirms the position which I had ventured to give to the sun relatively to some of the brighter stars, namely as belonging to the least fervid of the white stars, and just above those which begin to show a yellow light.

There are indeed some stars in the spectra of which the line D_3 which is seen in the prominences, and in the lower parts of the corona of the sun, appears as a bright line, but this may be due to gas below any true corona, which may be about these stars.

There are also the so-called nebulous stars, which are surrounded by an aureole of faint light of measurable angular extent, but it would seem more probable that these belong to, and should be discussed with, the clusters and nebulæ, and should not be regarded as exhibiting a corona of the nature of that which surrounds the sun.

So far then as our present powers of observation go, the corona of the sun stands alone; it is therefore the more to be regretted that the observations of this object are beset with great and peculiar difficulties. The absorption and scattering of the sun's light by our atmosphere, amounting according to Professor Langley to nearly 40 per cent.,* which are essential to the maintenance of the conditions under which life, as it now exists, is possible upon the earth, comes in, in this case, so seriously to our disadvantage that the corona can be seen for a few minutes only at long intervals. It is only on the rare occasions when the moon coming between us and the sun cuts off the sun's light from the air at the place where the eclipse is total, that we can see the corona through the cone of unilluminated air which is in shadow. On an average once in two years, for from three to six minutes, the corona is visible, and then only over a very narrow strip of the earth's surface. It is not surprising that many attempts have been made to observe the corona without an eclipse. The earlier attempts were based upon the hope that if the eye were protected from the intense direct light of the sun, and from all light other than that from the sky immediately about the sun, the eye might become sufficiently sensitive to perceive the corona. In the later attempts, success has been sought for from the great diminution of air-glare which takes place at high elevations, when the denser and more dusty parts of the atmosphere are left below the observer. Professor Langley made observations on Mount Etna, and also on Mount Whitney, 15,000 feet high. He says:—"I have tried visual methods under the most favourable circumstances, but with entire non-success." Dr. Copeland, assistant to Lord Crawford, observed at Puno at a height of 12,040 feet. In his report he says:† —"It ought to be mentioned that the appearances produced by the illuminated atmosphere were often of the most tantalising description,

* "Amer. J. of Science," September, 1884.

† Copernicus, vol. iii, p. 212.

giving again and again the impression that my efforts were about to be crowned with success."

The spectroscopic method by which the prominences may be seen without an eclipse, fails for the corona, because a small part only of the coronal light is resolved by the prism into bright lines, and of these lines no one is sufficiently bright and coextensive with the corona to enable us to see the corona by its light.

Let us look at some of the conditions of the problem. As the obstacle to our seeing the corona consists of the bright screen of illuminated air which comes in before it, it is of importance to consider the relative degree of brightness of the air-glare, under favourable conditions, to that of the corona behind it.

During the eclipse of 1878, Professor J. W. Langley found the apparent brightness of the coronal light at 1′ from the limb of the moon to be six times greater than that of the full moon, but at 3′ distance, the light to have fallen off to one-tenth of the light of the full moon.* Professor Harkness concludes for the same eclipse :—(1.) The total light of the corona was 3·8 times that of the full moon, or 0·0000069 of that of the sun. (2.) The coronal light varied inversely as the square of the distance from the sun's limb. (3.) The brightest part of the corona was 15 times brighter than the surface of the full moon. (4.) The corona of December 12, 1870, seems to have been 7¼ times brighter than that of July 29, 1878.† In his report on the eclipse of 1883, M. Janssen says :—"Cette expérience a montré qu'à Caroline l'illumination donnée a été plus grande que celle de la pleine lune."‡

The chief point of importance for this inquiry lies not so much in the actual value of the coronal light as in the relation of that value to the brightness of the illuminated air near the sun. Many observers have borne testimony to the continued visibility of the corona for some minutes (from three minutes to twelve minutes) after the end of totality.

The observations which give to us direct information on this point are those which have been made of the planets Venus and Mercury when they come in between us and the sun. It is obvious that as the planet approaches the sun it comes in before the corona and shuts off the light which comes from it. Under these circumstances the observer sees the sky in front of the planet to be darker than the adjoining parts, that is to say, the withdrawal of the coronal light from behind has made a sensible diminution of the brightness of the sky. It follows that the part of the sky about the sun, behind which the corona is situated, and to which its light is added, is brighter than

* Washington Observations. Reports of Solar Eclipses, 1878 and 1880, p. 214.

† Ditto, p. 392.

‡ "Annuaire pour l'An 1884" (B. des Longitudes), p. 875.

the adjoining parts, in a degree not far removed from the eye's power of distinguishing adjacent areas which differ by a small degree of brightness.

If, therefore, by any method of observation even a small advantage could be given to the coronal light as compared with the air-glare, and, especially, if, at the same time, we could by any method accentuate the small difference of illumination, a method might be found by which the corona could be observed.

When the report of the photographs taken during the Egyptian eclipse of 1882 reached this country, I was led to conclude that the coronal light as seen from the earth was strong in the violet, and probably to some extent also in the ultra-violet part of the spectrum.

Apart from the question of the greater relative intrinsic intensity of the more refrangible region of the coronal light as a whole, or of any one of its components (its gaseous component gave bright lines in the violet region), there are two considerations which show us that the coronal light should be strong in the violet as compared with the air-glare near the sun.

The selective absorption of our atmosphere would cause the light scattered by it in the near neighbourhood of the sun to be relatively poor in this part of the spectrum; but there is a second cause acting in the same direction, which arises from the selective power of absorption of the sun's atmosphere.

The absorption which the photospheric light suffers from the solar atmosphere has been investigated by Professor Langley, Professor Pickering, and especially with great minuteness by Professor H. C. Vogel. Vogel found that while at the edge of the sun's disk the red light was reduced to 30 per cent. of its value at the centre of the disk, the violet light was reduced to 13 per cent.

Vogel sums up by saying that if the solar atmosphere were removed, the brightness of the violet part of the sun's light would be increased about three times, but the red light one and a-half times only.* The selective action would doubtless be more strongly marked beyond the visible limit.

The rapid increase of absorption near the sun's limb, in Vogel's observations, indicates a low and dense solar atmosphere. Professor Langley agrees in this view of the sun's atmosphere. He says, "The portion of the (sun's) atmosphere chiefly concerned in absorption, I have been led to believe, from several considerations, is extremely thin, and I am inclined to think is mainly identical with the reversing layer at the base of the chromosphere."

Professor Hastings also considers the "layer which produces absorp-

* "Spectralphotometrische Untersuchungen." "Monatsbericht der K. Ak. d. Weissenschaften." Berlin, März, 1877. Also "Ueber die Absorption der chemicale wirksamen Strahlen in der Atmosphäre der Sonne," *ibid.*, Juli, 1872.

tion to be very thin," but he prefers to regard this layer as consisting not of gas, but of "a thin smoke-like envelope of precipitated material."* Professor Pickering assumes the existence of an absorbing atmosphere about equal in height to the sun's radius, but we shall see further on that there are reasons which make this supposition extremely improbable.

The light emitted by the corona, whether by the incandescent particles or by the gas mingled with them, which lies outside the low region of absorption, will not have been subjected to the same selective absorption as the photospheric light which is emitted below this region. For this reason the light emitted by the corona will be richer in the more refrangible rays than the sun's light before it enters our atmosphere, and will be in a still larger degree richer in these rays than the solar light which has been scattered by our atmosphere near the sun. These considerations led me to hope that if the corona were observed by this kind of light alone it would be at some advantage relatively to the air-glare which comes in before it. It was of importance at the same time to magnify the small advantage the coronal light might have by some method of observation which could bring out strongly minute differences of illumination. Such a power is possessed by a photographic surface. I took some pains to satisfy myself "that under suitable conditions of exposure and development a photographic plate can be made to record (strongly) minute differences of illumination existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtile as to be at the very limit of the power of recognition of a trained eye, and as it appeared to me, those which surpass that limit."†

* "Constitution of the Sun," "Amer. J. of Science," vol. xxi, p. 33.

† "Proc. Roy. Soc.," 1882, p. 411.

Professor Stokes has suggested the following method of increasing the intensity of that part of the coronal light which is polarised relatively to the glare from the sky. He says in a letter, which he permits me to add here:—"The light of the corona is known to be strongly polarised, while the atmospheric glare would show no sensible polarisation. Let p be the intensity of the coronal light along any radius vector which is polarised radially, and q the intensity polarised tangentially, and let $2a$ be the intensity of the glare. Then, without polarising the light, the intensity of the coronal light relatively to the glare would be as $p+q$ to $2a$. Suppose now the light falling on the plate to be polarised, say, in a north and south plane. Then north and south the ratio of the coronal light to that of the glare would be increased to p to a, while in east and west directions it would be reduced to q to a. In north-east and south-west as well as in north-west and south-east directions, the ratio would be the same as without polarisation. If in four successive photographs the plane of polarisation were set to north, north-east, east, south-east, we should get a relative increase in coronal light, in one or other of the photographs, all round the sun. It would be least in north-north-east, east-north-east, &c., directions, where it would be $p\cos^2 22\frac{1}{2}^\circ + q\sin^2 22\frac{1}{2}^\circ$ to a, or about $0{\cdot}85p + 0{\cdot}15q$ to a.

"The most convenient way of polarising would probably be to use a Nicol of some size not far from the plate."

In my early experiments I made use of coloured glass, or a cell containing a coloured liquid, for the purpose of isolating the violet part of the spectrum,* but afterwards I obtained the desired light-selection in the film itself by the use of argentic chloride, which Captain Abney had shown to be most strongly sensitive to light from *h* to a little beyond H.† Plates prepared with argentic chloride possess a further advantage for this work in consequence of the greater steepness of their gradations of density corresponding to differences of light-action as compared with argentic bromide plates.

When very small differences of illumination only, existing close about a body so enormously bright as the sun, have to be photographed, the most minute precautions have to be taken to avoid false effects upon the plate, which may arise from several causes. Lenses should not be used to form the sun's image on the sensitive surface, in consequence of possible false light about the image which may come from outstanding aberrations, though they have been corrected for photographic work, and from reflections at the surfaces of the lenses. I therefore employed a mirror of speculum metal. Other necessary precautions are described in my paper, namely, the position of the shutter very near the focal plane; protecting the sensitive surface from the sun's direct light by a metal disk a little larger than the sun's image; placing before the apparatus a long tube fitted with diaphragms to prevent light from the sky, excepting near the sun, from entering the apparatus; backing the plates with asphaltum varnish; and some others.‡

In my experiments in 1882 I used a Newtonian telescope by Short, but afterwards a fine mirror made by the late Mr. Lassell, which was so arranged that the image was formed directly upon the plate without reflection from a second mirror.§

About twenty plates were taken in 1882, in all of which an appearance more or less coronal in character is to be seen about the sun's image. After a very critical examination of these plates, in which I was greatly helped by the kind assistance of Professor Stokes and of Captain Abney, there seemed to be good ground to hope that the

* "Proc. Roy. Soc.," vol. xxxiv, pp. 411, 412.

† "Proc. Roy. Soc." vol. xxxiii, p. 175.

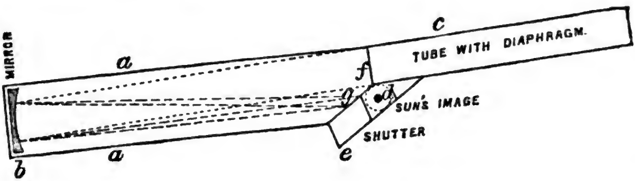
‡ "Proc. Roy. Soc.," vol. xxxiv, p. 409, also Report Brit. Ass., 1883, p. 348. See also the photographic experiments of Dr. Lohse, "Vierteljarsschrift Ast. Gesell.," Bd. xv, S. 134. Dr. Lohse sums up an account of his methods and results thus:—"Es gelang aber dieselben (die Schwierigkeiten) zu überwinden und Resultate zu erhalten welche zu einer Fortsetzung der—hier freilich selten möglichen und mit grösserem Vortheil in möglichst hoher Lage anzustellenden—Experimenten ermuthigen."

§ "I am indebted to Miss Lassell for the loan of a 7-foot Newtonian telescope made by the late Mr. Lassell. The speculum, which is 7 inches in diameter, possesses great perfection of figure, and still retains its original fine polish. I decided not to

corona had really been obtained upon the plates. On one plate especially forms resembling closely in character those present in the photographs of the eclipse of that year were visible.

In the following year, 1883, working with the Lassell mirror, I found that when the sky was free from clouds, but whity from a strong scattering of the sun's light, the sun's image in the photographs was well defined upon a sensibly uniform surrounding of air-glare, but without any such sudden increase of illumination near the sun's limb, or other indication which might be due to the corona. It was only when the sky was exceptionally clear that coronal appearances presented themselves with more or less distinctness.

use more than 3½ inches of the central portion of the speculum, partly for the reason that a larger amount of light would be difficult of management, and partly because this restriction of the aperture would enable me to adopt the arrangement which is shown in the diagram.



"It will be seen at once from an inspection of the diagram that in this arrangement the disadvantage of a second reflection by the small mirror is avoided, as is also the mechanical inconvenience of tilting the speculum within the tube as in the ordinary form of the Herschelian telescope. The speculum *b* remains in its place at the end of the tube *a*, *d*. The small plane speculum and the arm carrying it were removed. The open end of the tube is fitted with a mahogany cover. In this cover at one side is a circular hole, *f*, 3¼ inches in diameter, for the light to enter; below is a similar hole, over which is fitted a framework to receive the 'backs' containing the photographic plates, and also to receive a frame with fine ground glass, for putting the apparatus into position. Immediately below, towards the speculum, is fixed a shutter, with an opening of adjustable width, which can be made to pass across more or less rapidly by the use of india-rubber bands of different degrees of strength. In front of the opening *f* is fixed a tube, *c*, 6 feet long, fitted with diaphragms, to restrict as far as possible the light which enters the telescope to that which comes from the sun and the sky immediately around it. The telescope-tube, *a a*, is also fitted with diaphragms, which are not shown in the diagram, to keep from the plate all light except that coming directly from the speculum. It is obvious that, when the sun's light entering the tube at *f* falls upon the central part of the speculum, the image of the sun will be formed in the middle of the second opening at *d*, about 2 inches from the position it would take if the tube were directed axially to the sun. The exquisite definition of the photographic images of the sun shows, as was to be expected, that this small deviation from the axial direction, 2 inches in 7 feet, does not affect sensibly the performance of the mirror. The whole apparatus is firmly strapped on to the refractor of the equatorial, and carried with it by the clock motion."—Report B. Ass. Advanc. Science, 1883 p. 346.

The total solar eclipse of May 6, 1883, furnished an opportunity of comparing the photographs taken with an eclipsed sun with those taken in this country without an eclipse. On the day of the eclipse the weather was bad here, but plates taken before and after the eclipse were placed in the hands of Mr. Wesley, who had had much experience in making drawings from photographs taken during former eclipses. Mr. Wesley drew from these plates before any information reached this country of the results obtained at Caroline Island; he was, therefore, wholly without bias in the drawings which he made from them. When these drawings were compared afterwards with the Caroline Island plates, the general resemblance of the corona was strong, and the identity of the object photographed in England and at Caroline Island seemed placed beyond doubt by a remarkably formed rift on the east of the north pole of the sun, which is seen very nearly in the same position in the English plates and in those taken during the eclipse. This rift, slightly modified in form, was found to be present in a plate taken about a solar rotation-period before the eclipse, and also on a plate taken about the same time after the eclipse.*

In 1884, a grant from the fund placed annually by the Government at the disposal of the Royal Society was put into the hands of a committee appointed by the Council of the Royal Society for the purpose of photographing the corona at a place of considerable elevation. The Committee selected the Riffel, near Zermatt, which has an elevation of 8,500 feet, and appointed as photographer Mr. Ray Woods, who, as assistant to Professor Schuster, had photographed the corona during the eclipse of 1882, and who in conjunction with Mr. Lawrance had photographed the eclipse of the following year at Caroline Island.

Unfortunately during this year a very large amount of scattered light was always present about the sun, arising, it would seem, from

* Report B. Ass. Advanc. Science, 1883, p. 348, and Plates XI and XIA.

It seems desirable to put on record here a letter written by Mr. Lawrance to Professor Stokes, dated September 14, 1883:—"Dr. Huggins called upon Mr. Woods this morning and showed us the drawings Mr. Wesley has made of his coronas. He told us that he particularly did not wish to see our negatives, but that he would like us to compare his results with ours. We did so, and found that some of the strongly marked details could be made out on his drawings, a rift near the north pole being especially noticeable; this was in a photograph taken on April 3, in which the detail of the northern hemisphere is best shown, while the detail of our southern hemisphere most resembles the photograph taken on June 6; in fact, our negatives seem to hold an intermediate position. Afterwards I went with Dr. Huggins and Mr. Woods to Burlington House to see the negatives. The outline and distribution of light in the inner corona of April 3 is very similar to that on our plate which had the shortest exposure; the outer corona is, however, I think, hidden by atmospheric glare. As a result of the comparison I should say that Dr. Huggins' coronas are certainly genuine as far as 8′ from the limb."

finely-divided matter of some sort in the higher regions of the air. Mr. Woods observed from the Riffel that when no visible cloud or mist was present, there came into view a great aureole around the sun, about 44° in diameter, of a faint red colour at the outside and passing into bluish-white near the sun. This was clearly a diffraction phenomenon showing the presence of minute particles of matter of some sort in the higher regions of our atmosphere.

The abnormally large amount of air-glare from this cause—even on the finest days—prevented any success in photographing the corona in this country, and went far to counteract the advantages of being above the denser strata of air which Mr. Woods would have gained on the Riffel under ordinary circumstances.

Mr. Woods sums up his results in the following words:—"Results on the same day are almost, if not quite, alike both with the disk and without. The corona varies more or less from day to day. The clearer the day the better the results."*

During the last two years the sky in this country has been too bright from scattered light to make it possible to obtain successful photographs of the corona.†

We have now to discuss the probable nature of the corona.‡

The drawings, but especially the photographs, of the solar eclipses of the last twenty-five years show that notwithstanding great changes in form and in brightness, the corona is permanent in its more fundamental characters. The observed changes in form, in brightness, and in relative extension, are obviously due to secondary modifications of the conditions to which the corona owes its existence.

The circular form which was ascribed to the corona in the older observations can scarcely be regarded—even in the roughest sense of the word—as correct. On the contrary, the apparent form of the corona is always very irregular, in consequence of the greater extension and the greater relative brightness of certain parts. Upon the whole, there is

* "The Observatory," December, 1884, p. 378.

† It may not be unnecessary to state that what the photographer has to seize upon on his plates is the small excess of photographic power of the air-glare increased by the coronal light from behind over that of the air-glare alone. For this purpose the greatest care is necessary to select the most suitable time of exposure, and to arrest the slow development of the plate at the proper moment. Unless the attempt is made at a high elevation, the impression upon the plate must be a very slight one, and the developed image can only be seen under favourable conditions of illumination. Great care must be taken that all instrumental effects have been carefully eliminated. A convenient way of distinguishing effects upon the plate which are due to the instrument, is to take pictures with the instrument alternately on the west and on the east side of the meridian.

‡ The principal points of the discussion of the nature of the corona which follows were suggested in a discourse given at the Royal Institution, February 22, 1885, entitled "On the Solar Corona."

an observed tendency of the brighter parts of the corona to assume a square form in consequence of the greater extension of the coronal matter at the latitudes between the poles and the equator of the sun—that is, over the zones of maximum spot-action. The corona is frequently less extended over the poles and over the equatorial regions of the sun.

A noticeable exception to this state of things occurred in 1878, when the most remarkable features of the corona were two extended equatorial rays which could be traced to a distance of several solar diameters. We shall have to consider, further on, some circumstances which may have had a large influence in bringing about this state of the corona.

In addition to these large changes in the external form of the corona, there is a complex structure within it which appears to be in continual change. This inner structure was observed by Professor S. P. Langley in 1878, under very favourable conditions, with a telescope of 5 inches aperture and a power of 50.

He sums up his observations in the following words:—*

1. Extraordinary sharpness of filamentary structure.
2. Arrangement not radial, or only so in the rudest sense.
3. Generally curved, not straight lines.
4. Curved in different directions.
5. Very bright close to the edge, fading out rapidly, fading out wholly (this part of corona) at 5′ to 10′ from it.

In addition to this more minute structure, there are large bright portions, apparently streaming outwards, and often leaving between them less bright spaces, which have the appearance of rifts. There are also curved forms which seem to turn round and to return to the sun.

We must not forget that the corona has thickness as well as extension, and that the forms seen by us must appear more or less modified by projection. Rays inclined towards or from the observer would be materially altered in respect of their apparent position on the sun, and long rays in the nearer or more distant part of the corona would appear to start from parts of the sun other than those to which they really belong. For the same reason the increase of intrinsic brightness of the corona towards the sun's limb must be much less than the increase of brightness as seen by us, of which no inconsiderable part must be due to the greater extent of the corona in the line of sight as the sun is approached. Besides the real changes in the corona which have been observed at different eclipses, there are several sources of apparent change which may have modified the photographs taken of the corona. Of these may be mentioned—the state of the air at the time; the kind of sensitive surface employed; the length of exposure; whether

* Report T. S. Eclipse, 1878, Washington, p. 209.

the image has been formed by a lens which shortens and enfeebles the ultra-violet part of the light, or by a mirror which furnishes an image more truly representing the corona in the nature of the light existing in it. The difficulties which seemed to lie in the way of a satisfactory explanation of the forms and of the enormous extent of the corona, caused some doubts to be entertained as to the corona being a true solar appendage, and various views were formerly put forward to endeavour to explain the corona as an optical appearance only, arising from our atmosphere, from a lunar atmosphere, or from cosmical dust. Mr. De la Rue, in his address before Section A of the British Association in 1872, says truly, "The great problem of the solar origin of that portion of the corona which extends more than a million of miles beyond the body of the sun, has been, by the photographic observations of Colonel Tennant and Lord Lindsay in 1871, finally set at rest, after having been the subject of a great amount of discussion for many years."*

These earlier views are too completely a part of the history of the subject to need mention here, but for the circumstance that Professor Hastings has recently revived the theory of Delisle, that the corona is an optical appearance due to diffraction.

Professor Hastings bases his theory upon the behaviour of the bright line, 1474, which he saw, in his spectroscope, change in length east and west of the sun during the progress of the eclipse at Caroline Island in 1883. He assumes, in his explanation of this observation, that Fresnel's theory of diffraction may not apply in the case of a solar eclipse, and he suggests that at different moments the phases of the light waves may change so that they no longer form a continuous periodic series, and that it is possible, at such great distances, that the interior of the shadow may not be entirely dark, and that sufficient light may come inside to give to an observer on the earth the appearance of a bright aureole around the moon.†

Professor Hastings considers the simpler explanation of his observation which has been suggested, that the change in length of the line which he observed might be due to a scattering by our air of the light from the brighter part of the corona, and, therefore, might not indicate any change in the corona itself during the progress of the eclipse, to be untenable, on the ground that the air was too clear, and "diffusion absolutely insensible." He supports this strong statement by saying that the photographs taken by the English and by the French observers showed a sensibly black moon, and that "in the photograph of the coronal lines, H and K, taken by the English observers these lines ended abruptly at the moon's edge."‡

* Report B. A. Advanc. Science, 1872, p. 6.

† Report of Expedition to Caroline Island, 1883, Washington, p. 105.

‡ *Idem*, p. 107.

Captain Abney, F.R.S., who has the photographs taken at Caroline Island under examination, informs me that:—

"The diffusion during that eclipse was not insensible, as the lines H and K are distinctly visible across the black moon as dark lines. It is true that H and K, as bright lines, do stop at the moon's limb, but these lines are not coronal lines, as they belong to the prominences. In the Egyptian eclipse—which was a very short one—the prominences were far over the moon's limb, and the diffusion due to the atmosphere was such that the lines H and K were shown as bright lines over the moon. In the Caroline Island eclipse the prominences were much less marked and more hidden during the eclipse than was the case in Egypt, and it appears that the diffusion by the air must have been much greater in the former (Caroline Island) than in the latter, since it is the light from the hidden sun which was evidently reflected and re-reflected. On one side of the moon's limb H and K are seen reversed, whilst on the other they are reversed beyond the bright lines.

"In both cases the reversals are rather faint, but as strong as the reversal which was seen on the corona spectrum at the Egyptian eclipse. In my opinion, in the photographs of the corona with the longest exposure (I am not now speaking of spectrum photographs) the moon is not shown as perfectly black, but I should not like to found any theory very definite as to this, as it might be due to over-development, but I think not."

It should be mentioned that during the time that Professor Hastings observed the change in length of the line 1474, photographs of the corona were taken by M. Janssen, and by Messrs. Lawrance and Woods. M. Janssen says: "Les formes de la couronne ont été absolument fixes pendant toute la durée de la totalité."* The photographs taken by Messrs. Lawrance and Woods show that the corona suffered no such alterations in width and form as would be required by Professor Hastings' theory, during the passage of the moon across the sun.

For other points raised by Professor Hastings, and for his discussion of former spectrum observations of the corona, I must refer to his memoir.†

The evidence in favour of the corona being a true solar appendage appears to me to be of overwhelming weight. It seems difficult on any other hypothesis to explain satisfactorily—(1) the observed and the photographed spectra of different parts of the corona; (2) the visibility of the planets Venus and Mercury as dark bodies when near the sun; (3) the filamentous, and especially the peculiar curved structures seen in photographs of the corona; (4) the close agreement of photographs taken at different times during an eclipse, and

* "Annuaire pour l'An 1884," p. 859.

† *Vide ante.*

especially between photographs taken during the same eclipse at places many hundreds of miles apart.

At the same time a very small part of the light which is seen about the eclipsed sun must be due to diffusion by our atmosphere of the coronal light itself, especially of the very bright part near the sun's limb; and we have an indication of the amount of this diffused light from the apparent illumination of the dark moon, where the effects of diffusion will be most strongly present. During some eclipses the part of the sky where the sun and moon are may be faintly illuminated by light reflected from those regions of the atmosphere near the horizon which are still in direct sunlight.

It may be well to mention the principal hypotheses which have been suggested in explanation of the corona.

1. That the corona consists of a gaseous atmosphere resting upon the sun's surface and carried round with it.

2. That the corona is made up wholly or in part of gaseous and finely divided matter which has been ejected from the sun, or received by it, and which is in motion about the sun from the forces of ejection, of the sun's rotation, and of gravity, and possibly of a repulsion of some kind.

3. That the corona resembles the ring of Saturn, and consists of swarms of meteoric particles revolving with sufficient velocity to prevent their falling into the sun.

4. That the corona is the appearance presented to us by the unceasing falling into the sun of meteoric matter and of the débris of the tails of comets.

5. That the coronal rays and streamers are, at least in part, meteoric streams strongly illuminated by their near approach to the sun, neither revolving about nor falling into the sun, but permanent in position and varying only in richness of meteoric matter, which are parts of eccentric comet orbits. This view has been supported on the ground that there must be such streams crowding richly together in the sun's neighbourhood.

6. The view of the corona suggested by Sir William Siemens in his solar theory.*

The sun must be surrounded by a true gaseous atmosphere of relatively limited extent, but there are several considerations which forbid us to think of a solar atmosphere, in the proper sense of the term, that is of a continuous mass of gas held up by its own elasti-

* Since this lecture was read my attention has been called to the papers by Professor O. Reynolds, "On the Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena," and "On an Electrical Corona resembling the Solar Corona," in vol. v, 3rd Ser., "Lit. and Phil. Soc.," Manchester, pp. 44–56, and pp. 202–209. Professor Reynolds considers the solar corona to be a species of that action known as the electric brush, and to be well represented by discharging electricity from a brass ball in a partially exhausted receiver.

city, which rises to a height sufficient to afford an explanation of the corona which streams several hundred thousand miles above the photosphere.

Gravitation on the sun is about twenty-seven times as great as on the earth, and an atmosphere extending to a moderate coronal height, even if it consisted of a gas thousands of times lighter than hydrogen, would have more than metallic density at the sun's surface, a state of things which spectroscopic and other observations show cannot be the case.

There is another consideration from the rapid increase of density which would take place sunwards in such an atmosphere. Each stratum would be compressed by the weight of all the strata above it, and therefore in descending by equal steps the density of the atmosphere would increase in geometrical ratio. Professor Newcomb gives as an example an atmosphere of hydrogen; such a gas, though heated to as high a temperature as is likely to exist at a height of a hundred thousand miles above the sun, would double its density every five or ten miles.* There is no approach to so regular and so rapid an increase of density to be observed in the corona.

Another circumstance which puts a continuous gaseous atmosphere out of question is the fact that comets have passed unscathed through the coronal regions. Shooting stars passing with the relatively small velocity of thirty or forty miles per second through our atmosphere, rarefied as it is, at the height of fifty or sixty miles, are instantly burnt up. Resistance and heat increase as the square [or more probably for such high velocities as the cube†] of the velocity, yet the nucleus of a comet has passed through several hundred thousand miles of coronal matter with a velocity of 300 miles per second without suffering any sensible loss of velocity. These considerations are amply sufficient to show that the theory of a solar atmosphere of gas of the extent of the corona held up by its own elasticity cannot be entertained.

As we have reason to believe that the corona is an objective reality about the sun, matter of some sort must exist wherever the corona is seen to extend. The questions before us are—(1.) In what form does the matter exist? (2.) Whence does it come? (3.) What are the dynamical conditions under which it can exist at such great heights above the sun?

(1.) On the first of these questions as to the condition of the matter, the spectroscope has given us definite information.

The spectrum of the corona is compound, and consists of three superposed spectra.

(*a.*) A bright continuous spectrum, which informs us that it comes from incandescent solid or liquid matter.

* "Popular Astronomy," p. 259. † See Bashforth, "Phil. Trans.," vol. 158, p. 417.

(*b.*) A solar spectrum, which shows that the incandescent solid or liquid matter of the corona reflects to us light from the photosphere.

(*c.*) A spectrum of bright lines, which is relatively faint and varies greatly at different eclipses. We shall consider this spectrum more particularly further on; it is sufficient at this part of the argument that we speak of this spectrum so far only as it tells us of gaseous matter which accompanies the solid or liquid matter.

It is scarcely necessary to say that solid or liquid matter can exist in the corona only in the form of discrete particles of extreme minuteness.

The corona must, therefore, consist of a fog, in which the particles are incandescent, and in which the gaseous matter does not form a continuous atmosphere. Some of the considerations we have already had before us, make it evident that this coronal fog, except very near the sun, must be of a degree of tenuity surpassing any experience we possess from terrestrial experiments. In order to give some definiteness to our conceptions, let us suppose a single minute liquid or solid particle in each cubic mile. A fog even so extremely attenuated, or even much more so, would probably be fully sufficient to give rise to the corona, under the enormous radiation to which it is subjected.

(2.) The next question we have to consider is whether the matter of the corona is of solar origin, or whether it comes upon the sun from without.

Two external sources of the coronal matter have been suggested, and are widely held, namely:—(*a*) meteroids, and (*b*) the lost matter of the tails of comets.

(*a.*) The solar system is crowded with meteoroids revolving in all kinds of erratic orbits, and we know that the earth encounters many thousands of them every hour, but the sun is in a different position with regard to such of these bodies as belong to our system. In order to fall into the sun, the planetary meteoroids would have to be thrown into it, through some disturbance of their orbits, produced by planetary attraction, or by collision with other streams, unless we admit a slow retardation of velocity produced by a resisting medium.

There may be meteoroids which fall directly into the sun from space. Mr. Denning's recent observations seem to show that the solar system encounters meteor streams which may be moving with great velocity through space.*

Many of these bodies may fall into the sun, but we have no knowledge of conditions which would ensure so steady an inflow of meteoroids as would be needed to maintain the observed extent of the corona in a state of permanence about the sun.

(*b.*) The other suggestion which has been made regards the corona

* "Month. Not. R. Ast. S.," vol. xlv, p. 93. See also subsequent papers.

as fed by the lost matter of the tails of comets. We must remember that the matter which comes from the nucleus of a comet, and forms its tail, and is then lost, has been shown by the spectroscope to consist in nearly all comets, of carbon, hydrogen, nitrogen, and possibly oxygen.* If this matter is condensed into the discrete particles which form the tail, in the same conditions of chemical combination as it existed originally in the nucleus, we should expect these particles to be again vaporised in their near approach to the sun; and under these conditions we should expect to find the corona to be mainly gaseous, and to give a spectrum similar to that which is produced by the emitted light of comets. We know that such is not the case; there is, however, a single observation by Tacchini at Caroline Island, in which in one part of the corona he suspected two of the bands which are present in the ordinary cometary spectrum. His words are:—"Dans le spectre du grand panache, qui était faible et presque continu, et que l'on voyait seulement à fente large, j'ai observé deux bandes qui m'ont semblé être analogues à celles que j'ai observées dans les spectres de comètes, c'est-à-dire, la centrale et la moins réfrangible."† The terms in which this unique observation is given show that the lines, even if truly present, were faint and exceptional, and cannot be regarded as characteristic of the coronal light.

It may indeed be suggested that the cometary matter suffers decomposition at the time when it becomes luminous near the nucleus, and that carbon may be separated in a finely divided state, and go to form part of the lost matter of the tail. In the case of comets which have more than one tail, or exhibit rays driven off with a curvature different from that of the principal tail, there is good reason to believe, as Bredichin has endeavoured to show,‡ that each tail or caudal ray consists of matter different in density, which has been separated by a force of repulsion varying as the surface. It would appear doubtful, even on this view, if the supply of comets' tails is sufficiently regular in amount to maintain a permanent corona about the sun.

It seems to me to be much more probable that the corona is fed from the sun itself. This view is supported by the spectroscopic evidence, for the coronal gas is shown to consist of substances which exist also in the photosphere. The structure seen in the corona is much more in harmony with the view that the matter is going up from the sun, than that it is coming down upon the sun.

An examination of the photographs taken at eclipses, or of Mr. Wesley's admirable drawings from them, can scarcely fail to lead an unbiased student to the same conclusion as that which was forced

* "Phil. Trans.," 1868, p. 559, and "Proc. Roy. Soc.," vol. xxxiii, p. 1.

† "Annuaire pour l'An 1884," p. 862.

‡ "Annales de l'Observatoire de Moscou," and "Astron. Nachr.," No. 2411.

upon Mr. Lewis Swift when he observed the corona of 1878:—"I was irresistibly led to the conclusion that the corona, whatever may be its nature, is not a solar atmosphere, nor an inflow of meteoric matter, as many suppose, but rather an outflow of something."* These considerations appear to me to be of great weight in support of the view, that though some meteoroid and some cometary matter may fall into the sun, the corona consists essentially of matter coming from the sun.

(3.) We have now to consider under what dynamical conditions matter coming from the sun can take on forms such as those we see in the corona, and can pass away to such enormous distances, in opposition to gravitation, which is so powerful at the sun.

There is another celestial phenomenon, very unlike the corona at first sight, which may furnish us with a clue to the true answer to this question. The head of a large comet presents us with luminous streamers and rifts and curved rays, which are not very unlike on a small scale some of the appearances which are always present in and are peculiar to the corona. We do not know for certain the conditions under which these cometary phenomena take place, but the only theory upon which they can be satisfactorily explained, and which now seems on the way to become generally accepted, attributes them to electrical disturbances, and especially to a repulsive force acting from the sun, probably electrical, which varies as the surface, and not like gravity, as the mass.† A force of this nature in the case of highly attenuated matter can easily master the force of gravity, and as we see in the tails of comets, blow away this thin kind of matter to enormous distances in the very teeth of gravity.

If such a force of repulsion, acting from the sun, is experienced by comets, it must also be present near the sun, and may well be expected to show its power over the matter ejected from the sun's surface.‡

* Report Total Solar Eclipses of 1878 and 1880, Washington, p. 231.

† "Proc. Roy. Inst.," vol. x, p. 9; also Bredichin, "Annales de l'Observatoire de Moscou," vol. v, No. 2, p. 39; "Astr. Nachr.," No. 2411; and papers by Faye in the "Comptes Rendus." Stokes, "On Light as a Means of Investigation," p. 70 *et seq.* O. Reynolds, "On Cometary Phenomena," "Mem. Lit. and Phil. Soc.," Manchester, vol. v, p. 192.

‡ As a contribution to the history of opinions involving more or less distinctly the idea of repulsion, it may be well to give the following words by Professor Young ("Amer. J. of Science," vol. i, May, 1871, p. 7):—"On the one hand, that of Professor Norton and Mr. Proctor, whose views regarding these rays (the *long* faint rays) are nearly identical, and represent them to be streams of matter, similar to cometary substance or auroral." In a foot-note Professor Young says further:—"Since my name has sometimes been referred to in connexion with the so-called 'Auroral Theory of the Corona,' it is proper for me to state that I make no claim to be its originator. So far as I know, Professor Norton was the first to work out and publish a connected theory of the subject, basing his conclusions largely upon

The existence of a force, which, under suitable conditions, may become one of repulsion at the sun's surface, is not hypothetical only, for we have reasons to believe that such a force must really be present there. Though we have no definite knowledge of the distribution of electricity on the surface of the sun, we do know that chemical and mechanical actions are taking place there which must be accompanied by electrical disturbances. It seems to me that these disturbances, which must be of a high order of magnitude, bring about the magnetic changes on the earth which are observed to take place in connexion with solar phenomena.*

The grandest displays of terrestrial electrical disturbance must be altogether insignificant in comparison with the electrical changes which must accompany the ceaseless and fearful activity of the photosphere. Not to mention the frequent outbursts of heated gas thousands of miles high, and over areas in which the earth could be engulphed, there is the unceasing formation of the fiery photospheric cloud-granules about as large as Great Britain. Surely it is not too much to say that our terrestrial experience of lightning and of

his discussions of Donati's comet, which were printed in this Journal some years ago. Professor Winlock also informs me that he has held and published a very similar opinion, and so I think have more than one of the European astronomers. My own father, more than twenty years ago, was accustomed to teach from the same chair of astronomy which I now occupy, an essentially similar doctrine. Thus the idea had long been familiar to me, and, I presume, more or less so to astronomers generally."

It may be well to give a more direct reference to the papers of Professor W. A. Norton. He says, speaking of comets ("Proc. Amer. Ass.," 1854, p. 166):—"The tails of comets flowing away under a repulsive force from the sun." . . . . . "[this repulsive force] to consist of the impulsive action of auroral matter flowing from the sun." Again, speaking of the corona, he says: "The aigrettes of auroral matter flowing off chiefly from the polar regions into space." In a subsequent paper ("Proc. Amer. Ass.," 1859, p. 167) Professor Norton defines his idea of the repulsive force as "a general force of cosmical repulsion exerted by all cosmical masses."

Mr. Proctor's views will be found in his work, "The Sun, the Ruler of the Planetary System," 3rd Ed., 1885, pp. 326—427.

For M. Faye's views on a repulsive force, see "Annuaire pour l'An 1883," also "Annuaire pour l'An 1885," and numerous papers in the "Comptes Rendus." Reference should also be made to the conjectures on the existence of a repulsive force thrown out by Sir John Herschel in his Cape Observations.

* Professor Stokes and Professor Balfour Stewart have both speculated on the connexion between solar disturbances and terrestrial magnetism, and have both imagined that the operative solar change is thermic—not electrical, and that it is through radiation that it affects the condition of the earth in such a manner as to be manifested by magnetic disturbances, though the modes in which these philosophers have conjectured that this takes place are wholly different. In a subsequent note I have suggested that the operative solar change is electrical, and that the action is probably one of statical induction.—August 20, 1885.

auroræ fail to supply us with any adequate basis for a true conception of the electric forces in action on the sun.

The phenomena of comets show not merely a highly electrical condition of the sun's surface, but also the permanence of an electric potential of the same kind, whether negative or positive.* Though we do not know enough of what is taking place at the sun to define the conditions which may cause the matter ejected from the sun's surface to have a high electric potential of the same name, yet we can see that broadly all the different actions which take place there, and to which the electric disturbances are probably due, are parts of one continuous process going on always in the same direction, namely, the transference of energy from the interior to the photosphere, and the loss of the energy there, in the radiant form.

We must bear in mind that a strongly electrified state of the solar surface would not act as a force of repulsion upon discrete particles of matter insulated from each other, such as exist in the tails of comets and in the corona, unless these particles possessed an electric potential of the same kind as the solar surface. If these particles were in an unelectrified condition, the action of the sun would be one of statical induction only, altering the original distribution of electricity over their surfaces, but powerless to change in any sensible manner the positions of their centres of gravity in space, because the attraction on one side of each particle would be balanced by the repulsion on the other.

* The sun's potential may be regarded as due to actions of some kind always going on, or to a permanent charge received at some past time. The sun if once charged with electricity of the same name would doubtless remain so charged, as Mr. Crookes' experiments appear to show that a vacuum would be a perfect insulator.

If the sun has been charged with electricity of one name, we do not know how this came about, though more than one probable conjecture might be hazarded. Some facts mentioned further on as to the influence of Mercury and of Venus upon the coronal matter would seem to make it very probable that these planets are permanently charged with electricity of the other name to that of the sun. If this should hold good also of the more distant planets (we know nothing of the absolute potential of the earth's surface), we should have the planets charged with one kind of electricity, and the sun charged with the opposite electricity. As we have reason to believe that the sun and the planets formed originally one cosmical mass, the question may be suggested whether these changes of electricity of opposite names can have been brought about in connexion with the separation of the matter which forms the planets from that which exists in the sun.

If we regard the sun as possessing an electric potential of one name, it is not absolutely necessary to suppose the local electric disturbances which are spoken of in the text. Electric disturbances are undoubtedly taking place there, and through these the ejected matter might come to have a higher potential than it possessed as forming part of the sun. Through these local disturbances some of the matter of the corona might have sometimes a higher or lower potential of the same name, and in this way might arise one of the varying conditions upon which the observed changes in the corona depend.—August 20, 1885.

If we grant the existence of a high electric potential of the sun's surface, we become possessed of a means of explanation of the chief coronal phenomena, provided we accept the conclusion to which our arguments have already led us, that the matter of the corona is of solar origin.

The photosphere is the seat of ceaseless convulsions and outbursts of fiery matter. Storms of heated gas and incandescent hail rush upwards, or in cylones, as many miles in a second as our hurricanes move in an hour. Dante's and Milton's poetic imaginings of Hades fall far below the common-place scenes at the solar surface. Is it then going beyond what might well be, to suppose that some portions of the photospheric matter, having an electric potential of the same kind as that of the solar surface, from which they come, and ejected, as is often the case, with velocities not far removed from that which would be necessary to set them free from the sun's attraction, should come under the action of a powerful electric repulsion, and so be carried upwards, and from the sun ?

If we take this view of things, we are able to accept the objective reality of many of the very long coronal rays, which seem to rest upon sufficient testimony. At the eclipse of 1878, Professor Langley traced the coronal matter to a distance of twelve solar diameters, and he adds: "I feel great confidence in saying that (this distance) was but a portion of its extent."* Professor Newcomb traced this ray to about the same distance, "six degrees from the disk."† Such distances are small as compared with the extent of the tails of comets.

This view of the corona is in harmony with the source of the matter, and of the forces which the structure of the corona almost irresistibly suggests, namely, that these have their seat in the sun. We should expect, what we find to be the case, that there is usually great coronal richness and extension over the spot zones where the solar activity is most fervent. Matter blown upwards by an electrical repulsion would rise with the smaller rotational velocity of the photosphere from which it started, and would appear to lag behind in its ascent, and so give rise to the curved rays, which are so common a feature. We may well suppose that the forces of eruption and of subsequent electrical repulsion would vary in different places, and not be always strictly radial; under such circumstances a structure, similar to that which the corona presents, might arise. A force of repulsion would also be present among the similarly electrified particles of the corona, acting in all directions, and would cause these particles to separate from each other, during their ascent from the sun; the amount of this diffusion would depend upon several factors, among others, upon the original velocity of ascent, and upon the density and the degree of electric potential of the repelled stuff.

* Report Total Eclipse, 1878, Washington, p. 208. † *Idem*, p. 104.

A relatively very small amount of matter, under this diffusing force, would suffice to give rise to the corona, and we can see how the extremely attenuated state of the corona, consisting as it must do, of minute particles widely separated, it may be by miles each from the other, may have been rapidly brought about.

It is now time to consider the gaseous matter which we know to be associated with the coronal particles, but not to form a continuous gaseous atmosphere. The gas which exists with the incandescent particles, and which the spectroscope shows to have come from the photosphere, may have been carried up as gas, or have been in part distilled from the condensed matter which forms the coronal particles, under the enormous radiation to which they are exposed. Such a view of the gas which is present in the corona, would not be out of harmony with the circumstance that the amount of gas relatively to the incandescent particles appears to vary (at the last eclipse in Caroline Island it seems to have been but very sparingly present), nor with the very different heights to which different bright lines may be traced at different parts of the corona and at different eclipses. Gases of different vapour-density would be acted upon differently by an electric force of repulsion which varies as the surface, and would to some extent be winnowed from each other; the lighter the gas, the more completely would it come under the sway of repulsion, and so would be carried more rapidly to a greater height than a gas more strongly held down by gravity. The relative proportions, as well as the actual amounts, at different heights in the corona, of the gases which the spectroscope shows to exist there, would vary from time to time; they would depend in fact also on the largeness of supply from below, in other words, upon the state of activity of the photosphere, and in this way there would come about a relation probably between the corona and the prominences.

The varying amount of gas in different parts of the corona is illustrated by the following statement in the Report on the Eclipse of 1882, by Captain Abney and Professor Schuster:—

"The ring in the green (1474) is particularly strong in the south-western quadrant, and hardly visible at some other points of the sun's limb. The yellow ring (D_3) is much fainter on the whole, but more uniform all round the sun."

Further on (p. 270) they say—"As regards the corona, we may perhaps point out that hitherto the position of only one true coronal line had been fixed, though two other lines had been suspected. The corona during the late eclipse seems to have been especially rich in lines. Thollon observed some in the violet without being able to fix their position, and Tacchini could determine the position of four true corona lines in the red; from the photograph we have been able to

measure about thirty additional lines, thus increasing the number of lines considerably."*

Captain Abney informs me as follows: "The spectrum of the corona had fewer lines in the Caroline Island eclipse (1883) than in the Egyptian eclipse (1882), and the corona was much brighter at one limb than at the other in 1883. I think I can trace reversed Fraunhofer lines beyond the bright lines H and K away from the moon's edge."

It would seem probable that at the time of the eclipse of 1883, the amount of light-emitting gas was smaller relatively to the number of incandescent particles than at the time of the eclipse of the previous year. This supposition agrees with the fact that the scattered solar light, showing the Fraunhofer lines, was strong in 1883.

There may be another connexion between the corona and the prominences besides that of a supply of gaseous matter, namely, one due to an increase of electric potential of the ejected matter when the prominences are numerous and large.

The electric disturbances which accompany the formation of large sun-spots are well known to be of sufficient magnitude to be felt upon the earth, by causing changes in the distribution of the terrestrial magnetism sufficiently great to affect our instruments.† The Astro-

* "Phil. Trans.," 1884, pp. 264 and 270.

† We do not know the mode in which the sun acts upon our magnets. The solar action may be a direct one due to changes in the sun's magnetism, or to an electromagnetic action due to electric currents, or to electrified matter in motion with a high velocity. The views suggested in this lecture of the sun's electrified state, and of the nature of the corona may possibly throw some light on this point. Two distinct modes of the sun's action on the magnetic needle seem to be possible:—

(*a*.) The sun being a charged conductor separated from the earth, also a conductor, by an insulating vacuum, would affect the distribution of the earth's electricity by its power of statical induction. As the earth rotates currents would be set up about it to effect the redistribution of electricity required to satisfy the inducing influence of the sun. May we not find in these earth-currents an explanation of some of the phenomena of the earth's magnetism? However this may be, the changes in the sun's statical induction which would follow from the shooting forth of the electrified matter of the corona, may well so affect the earth-currents as through them to bring about the disturbances observed in the needle. The electrified matter of the corona which leaves the sun will still go on, even when too diffused to be visible, and will still continue to produce upon the earth the effect due to its charge of electricity. The amount of this action will depend greatly upon the direction of the projected matter relatively to the position of the earth.

(*b*.) The other possible mode of action of the corona would be to suppose an electromagnetic action upon the earth. The electrified coronal matter moving with a high velocity would act similarly in this respect to electric currents. Among other difficulties we must consider the rapid decrease of electromagnetic action at a distance.

If the sun is a charged body, then in consequence of continually parting with matter charged with electricity of the same name as that of the sun's charge, the sun's potential would be slowly decreasing. This consideration would be in support,

nomer Royal, writing of the magnetic activity of the year 1882, says: "The month of November, which was characterised by the appearance of a very large sun-spot, being particularly disturbed with remarkable magnetic storms on November 17, 19, and 20, and many interesting cases of lesser disturbance."*

We can scarcely doubt but that similar electric disturbances of exceptional magnitude accompany the formation of the prominences; indeed these phenomena may themselves be, in part at least, electric discharges analogous to terrestrial auroræ.† However this may be, we can scarcely doubt that large electric disturbances accompany them. Tacchini takes the view that electricity plays a chief part in the prominences, and believes that he is able to show a connexion between these phenomena and corresponding changes in the magnetism of our globe.‡

Hitherto in our discussion of the forces which may be active in the corona, we have taken account only of the influence of electrical changes which take place upon the sun. Now these changes at the sun make themselves felt upon the earth; we may then well suppose, with a high degree of probability, that the earth,§ and especially the

of the conjecture thrown out in the last sentences of the text, that the corona was formerly of larger extent, and that it will continue to diminish.—August 20, 1885.

[My attention has been called this day to a paper by Prof. O. Reynolds, "On the Electro-dynamic Effect which the Induction of Statical Electricity causes in a Moving Body. This Induction on the part of the Sun a probable Cause of Terrestrial Magnetism." "Mem. Lit. and Phil. Soc.," Manchester, vol. v, 3rd Ser., p. 209.—Sept. 29, 1885.]

* Report of the Astronomer Royal, 1883, p. 13.

† See Balfour Stewart, "Proc. Roy. Inst.," vol. iv, p. 60.

‡ "Reale Accademia dei Lincei (March 1, 1885), S.N., vol. i, p. 181. Tacchini says:—"Ciò viene anche a corroborare l'opinione mia e di qualche altro, che cioè nel fenomeno delle protuberance solari l'elettricità attia una parte rilevante, da dovere forse considerare non poche di esse come fenomeni puramente ellettrici, come aurore polari, capaci di indurre sul nostro globo i correspondenti disturbi magnetici; . . . . noi possiano intanto considerare come cosa assicurata alla scienza, che il fenomeno della maiehie solari, quelle delle protuberanze ed il magnetismo terrestro variano così dì accordo."

At the same sitting Professor Respighi took a different view (p. 174) and stated he did not consider the prominences to be of a nature to occur in periods, and that he could not admit a connexion between the maxima and minima of the prominences and the elements of terrestrial magnetism. At the following sitting, March 15 (p. 228) Tacchini replies to the objections of Respighi, and endeavours to show that Respighi has been influenced by his preconceived views of the nature of spots and prominences.

§ Mr. Broun, in his discussion of the variations of the earth's magnetism ("Proc. Roy. Soc.," vol. xxiv, p. 231), says:—"It is shown that those changes (in 1844 and 1845) occur at intervals of twenty-six days, or multiples of twenty-six days. . . As this period is that of the sun's rotation relatively to the earth, it appears to follow that the earth has some action on the sun, or (more probably) on some ray-like emanation from the sun, which causes these changes in the earth's magnetism."

nearer planets Venus and Mercury, exert an influence on the electrified and attenuated matter of the corona.

The elaborate researches of Mr. De la Rue and Professor Balfour Stewart appear to show an influence exerted by Venus and Mercury upon the solar regions of spot action.

We know nothing of the electric distribution on Venus and on Mercury, but it seems more than probable that these bodies, as well as the meteor-swarms nearer to the sun, have an influence in determining the mode of outflow of the electrified coronal matter in the directions in which they happen to be. The influence may be one of attraction, giving rise to coronal extension or rays from the corona, or to repulsion, in which case we might have what appears to us as a rift directed towards the body.

We have not sufficient data to furnish certain information on this point, but it may be of interest to quote the following sentences from Mr. Trouvelot's Report of the Eclipse of 1878*:—

"There is a fact connected with this eclipse, which, if not due to a singular coincidence, would seem to point to some attractive action of the planets on the solar atmosphere (corona). On the day of the eclipse Mercury and Venus were in almost opposite points of their orbits, with the sun between and almost on a line with them, while the Earth on the same day was in a part of its orbit which formed the apex of an equilateral triangle having for base the line joining Mercury and Venus. Knowing this, it is perhaps singular, and anyhow very remarkable, to see that the eastern wing of the corona was directed on a straight line to Mercury, while the western appendage was directed on a straight line to Venus. The coincidence was still greater. As in regard to the sun, the two planets were not exactly on the same line, Mercury being a little to the north, while Venus was a little to the south of the ecliptic; the solar appendages have shown the same peculiarity, their axes being a little inclined to each other."

I may say that the inclination of the axes of coronal extensions on the two sides of the sun may be seen in the photographs of this eclipse. It should be stated that Professor Newcomb, who observed the coronal extension towards Venus, says:—"I tried to judge whether the western one (ray) pointed towards the planet Venus, then plainly visible near the horizon. The direction was apparently very slightly below that of the planet." Professor Newcomb's words seem to show that he did not make any allowance for refraction, which would make the planet when near the horizon appear sensibly higher than its true place.

If sufficient evidence should be forthcoming in the future to establish a sensible influence of the planets upon the corona, we should not

* Page 93.

expect to see the coronal matter in all cases moving exactly towards or from a planet, because this matter would be also under the influence of a motion in the direction of its primary repulsion, and also of one of rotation about the sun.*

There has been some difference of opinion as to whether the corona is uniform in constitution from the sun's limb outwards, or whether it consists of two parts, which have been distinguished by the names, "inner corona" and "outer corona."

There can be no doubt that at certain times, and in certain solar latitudes, a lower part of the corona, such as that described by Professor Langley, extending from about 5′ to 10′, is so much brighter than the parts outside of it that it seems to form what may be called an inner corona. At the same time, the photographs of different eclipses, and Mr. Wesley's drawings from some of them, show distinctly that all the stronger indications of structure can be traced down almost to the sun's limb, and that the brighter parts within some 6′ to 10′ of the limb, are not equally bright all round the sun. This brighter inner part is represented very strongly in several drawings which accompany Mr. Stone's paper on the eclipse of 1874, especially in one by Mr. Wright.† There seems great probability that the corona

* General Tennant, F.R.S., informs me that since this lecture was read, he has calculated the places of Mercury, Venus, and Mars for the eclipse of 1871 and the eclipse of 1882. He says:—

"The positions at the eclipse of 1871 are—

Mercury	position angle	100° 39′
Venus	„	278° 40′
Mars	„	80° 40′

Mercury is thus not far from the direction of the great prominences lettered H and I (see catalogue at page 27 of my report, 'Mem. R. Ast. Soc.,' vol. xlii), and corresponds to the greatest extension of the coronal matter, namely, 45′ in my table. Venus is near the group lettered V, W, and X, of which group V is less only to H and I in height, and corresponds to the next greatest extension of the corona namely, 34′ 56″ in my table. The real heights of the visible extensions, allowing for the foreshortening, would be for Mercury, 41′ 3″ or 45′ 31″, according to the reading taken; and for Venus, 47′ 05″. Any such calculation, however, implies a form of the coronal extension which does not exist. The more foreshortened ray would, in fact, on account of its breadth, seem longer in proportion than the one which is seen more nearly perpendicularly to its axial direction; and in this case this consideration would tend to reduce the real extension of the Venus ray. Mars does not seem to have any marked ray directed to him, but any such ray would be much foreshortened if it existed.

"At the time of the eclipse of May 16, 1882 ('Phil. Trans.,' vol. clxxv, Plate 13), we should have the effects of the planets Mercury and Venus coincident, and not much foreshortened, in the coronal pictures. The combined effects of these planets are shown in the protruding angle at the upper left side of the engraved corona. There seems a marked protrusion of the general light thereabouts which would be opposite to the planets."—August 15, 1885.

† "Mem. Roy. Astron. Soc.," vol. xlii, pp. 43, 51, and 53.

is of the same nature throughout, but that there is often so much more matter, in other words, the coronal fog is so much denser, within 5′ to 10′ of the limb, that under the effects of projection, and when seen by the eye or with a very low power, this part of the corona appears to be marked off from the corona beyond. It is possible that a clue to the real state of things may be found in the photographs of the corona of 1878. When these are examined the long equatorial rays seen by eye, can be traced a little distance beyond the bright corona, but it is found that the corona, as a whole, is not drawn out at this part, so as to extend to several solar diameters, but that these great extensions consist of rays or streamers coming out from the general coronal mass, something in the way in which fainter rays often extend from the principal tail of a comet. They may be due to a similar cause, namely, the electric repulsion acting upon particles which are more completely under its sway, either from their less specific gravity, or their more highly electrified condition. The consideration presents itself, whether in this state of things, we have only an extreme case of the conditions always present in the corona, which gives rise to the appearances which have suggested the distinction of an "inner corona" and an "outer corona."

There is another question which awaits consideration, namely, whether the corona rotates with the sun. It seems obvious that if the corona is due to a supply of matter and to forces coming from the sun, then the coronal structure and the degree of extension, which are produced by them, at any part of the sun, would continue to be produced by these agencies at that part of the sun, and in that sense the corona would rotate. In the case of the more distant and diffused parts, the rotation could scarcely be of one and the same material object, any more than in the sweep of a comet's tail at perihelion, the corona being constantly renewed and reformed over each part of the solar surface. If we suppose the corona to come under the influence of an external force as that of a planet, then we should expect the ray drawn out towards it, or the rift formed opposite to it, to continue to be directed to this external object, and to be independent of the solar rotation. The subpermanence of any great coronal form, therefore, would probably have to be explained by the maintenance for some time of the conditions upon which the form depends, and not by an unaltered identity of the coronal matter; as in the case of a cloud over a mountain top, or of a flame over the mouth of a volcano.

We have to consider another question: What becomes of the coronal particles? Are they carried away from the sun, as the matter of the tails of comets is lost to them; or do they return to the sun?

The results of eye observations, as well as of the taking of photographs with different exposures, have shown that there is great probability that the corona has not an outer boundary, but that it is lost

in an increasing faintness and diffusion. The absence of a limit is probably true only of the faint outer parts of the corona. Within, and especially about the distance from the sun's limb to which the so-called "inner corona" usually extends, there is evidence of an apparent arrest of coronal matter, due in part probably to the effects of perspective, and within this distance are seen numerous rays which turn round and descend towards the sun. These returning curved forms are well shown in Mr. Wesley's drawings of the eclipse of 1871.*

We are led to the conclusion that many of the coronal particles return to the sun, but that in the case of other particles which form the stronger coronal rays and streamers, there is no return, but that they leave the sun, and, at the same time, separate more widely from each other by their mutual repulsion, and become too diffused to be visible. The state of extreme attenuation of this diffused coronal matter—such that the nuclei of comets pass through it without sensible retardation, enables us to see that the corona may be maintained at an extremely small expenditure of solar material. Among other considerations it may be mentioned that an electric repulsion can maintain its sway only so long as the repelled particles remain in the same electrical state; if through electric discharges the particles cease to maintain the electric potential they possess, there will be no longer any force of repulsion acting upon them, and gravity will be no longer mastered. If when this takes place, the particle is not moving away with a velocity sufficiently great to carry it from the sun, the particle will return to the sun. Of course, if the effect of any electric discharges or other local conditions has been to change the potential of the particle from positive to negative, or the reverse, as the case may be, then the repulsion would be changed into an attraction acting in the same direction as gravity.

This ceaseless outflow of extremely minute particles, very widely separated from each other, may possibly throw some light on another phenomenon which has not yet been satisfactorily explained, namely, the zodiacal light.

The views which I have ventured to put forward in this lecture would lead us to expect that a more extended and more brilliant corona surrounded the sun in early geological time, and that if the skies were then of their present degree of clearness, the corona would probably have been visible about the sun.

May the corona have been still faintly visible in the earliest ages of the human race? Are there any philological traces of it in the earliest words and ideas connected with the sun?

On those eastern plains, where the air is of so great purity, did early men still see faintly a true παρήλιος?

Similar considerations point to a slow secular diminution in extension

* "Mem. Roy. Astron. Soc.," vol. xli, Plates 6, 7, 8, and 10.

and in brightness of the corona, as the sun slowly loses heat, and the actions of the photosphere become less fervent.

The candle of the sun is burning down, and so far as we can see, must at last reach the socket. Then will begin a total eclipse which will have no end:

> "Dies illa
> Solvet seclum in favilla."

"Results of the Harmonic Analysis of Tidal Observations." By A. W. BAIRD, Major R.E., and G. H. DARWIN, F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received March 19, 1885.

The harmonic analysis of continuous tidal records, inaugurated by a Committee of the British Association in 1868, has now been carried out at a considerable number of ports. Some of the earlier results were collected together in the Reports to the Association in 1872 and 1876, and in a paper by Sir W. Thomson and Captain Evans, read before the Association in 1878, but the largest mass of data is contained in the tide tables now being annually published for the Indian ports under the authority of Her Majesty's Secretary of State for India.

The Report of the last Committee of the British Association, published in the volume for the meeting at Southport in 1883, is entirely theoretical, and has been adopted in India as a manual of the method of harmonic analysis. It is there shown how the results of the analysis are to be presented in a form appropriate either for theoretical treatment or for mechanical prediction by the instrument of the Indian Government in London. It is also shown how the scattered results, referred to above, may be reduced to the form which has been adopted as a standard. Major Baird has collected the whole of the Indian results, and those contained in the Reports of 1872 and 1876, and, by the aid of his staff of computers at Poona, has reduced them to this standard form. The greater part of the annexed tables is the result of this work.

We must refer to the Report to the British Association for 1883 for an explanation of the method of harmonic analysis, but it will be well to give a few words of explanation.

Each one of the tides, into which the oscillation of sea-level is regarded as analysed, is expressed in the form—

$$\mathrm{f H} \cos(V+u-\kappa).$$

$V+u-\kappa$ is the argument of the tide, and increases uniformly with the time, so that this term represents a simple harmonic oscillation of the sea-level with semi-range f H.

It is supposed that u stands for the mean value, estimated over the year or period of observation, of a certain known function of the longitude of the moon's nodes, or in a few cases of the sun's perigee; f stands for a factor of augmentation or diminution of the range of tide due to the variability of the obliquity of the equator to the lunar orbit, and a mean value for f estimated over the year or period of observation is adopted. Tables for computing u and f for each tide are given in the Report.*

V is a known linear function of the local mean time, of the mean longitudes of the sun, of the moon, and of the lunar perigee, and it increases uniformly with the time; the rate of its increase, measured say in degrees per mean solar hour, is called the speed of the tide.

The numerical operation of harmonic analysis gives us H and κ, which are constants peculiar to the port of observation. As the tide tables are principally for the use of British sailors, H is expressed in feet and decimals of a foot, and κ is an angle less than 360°. The argumen $V+u-\kappa$ is such that if the equilibrium theory of tides were true, with a water-covered globe, then κ would be zero; and κ divided by the speed is the time elapsing after any theoretical equilibrium high-water until the next actual high-water; we may call κ the "lag" of the tide. If the equilibrium theory were true, H would have a value which may be computed from the formulæ given in the Report.

If tidal observations were perfectly accurate, and if the tides were undisturbed by the weather, H and κ would be absolute constants for each tide and for each port, whatever periods are submitted to analysis; and in proportion as they are found to be constant so is the analysis satisfactory.

A knowledge of H and κ is necessary and sufficient to determine the height of water, as due to the particular tide, at any time, past or future.

The letters† γ, σ, η, ϖ have been appropriated to the earth's angular velocity of rotation, and to the mean motions of the moon, of the sun, and of the lunar perigee respectively. Hence the rate of increase of V or the speed of tide, is expressible by these symbols. For practical convenience an initial has been adopted to indicate each one of the tides; and we here reproduce Schedule A of the Report containing the arbitrarily chosen initial letters, the speed, and a descriptive name for most of the tides.

The tides involving γ in the speed are approximately diurnal, those containing 2γ are approximately semi-diurnal, and those containing 3γ, 4γ, &c., are approximately ter-diurnal, quater-diurnal, and so on. Those whose speed does not involve γ are called tides of long period, since the quickest of them has a period of a fortnight.

* In the case of the results for the English ports below it is Greenwich mean time.

† The initials of γῆ, σελήνη, ἥλιος, and perigee.

Schedule of Notation.

Initials.	Speed.	Name of Tide.
M_1 M_2 M_3 &c.	$\gamma-\sigma-\varpi$, and $\gamma-\sigma+\varpi$ $2(\gamma-\sigma)$ $3(\gamma-\sigma)$ &c.	Principal lunar series.
K_2	2γ	Luni-solar semi-diurnal.
N	$2\gamma-3\sigma+\varpi$	Larger lunar elliptic.
L	$2\gamma-\sigma-\varpi$ and $2\gamma-\sigma+\varpi$	Smaller lunar elliptic.
	$2\gamma+\sigma-\varpi$	Luni-solar elliptic semi-diurnal.
2N	$2\gamma-4\sigma+2\varpi$	Lunar elliptic, second order.
ν	$2\gamma-3\sigma-\varpi+2\eta$	Larger lunar evectional.
λ	$2\gamma-\sigma+\varpi-2\eta$	Smaller lunar evectional.
O	$\gamma-2\sigma$	Lunar diurnal.
OO	$\gamma+2\sigma$	
K_1	γ	Luni-solar diurnal.
Q	$\gamma-3\sigma+\varpi$	Larger lunar elliptic diurnal.
	$\gamma-\sigma-\varpi$ included in M_1	Smaller lunar elliptic diurnal.
J	$\gamma+\sigma-\varpi$	Luni-solar elliptic diurnal.
	$\gamma-4\sigma+2\varpi$	Lunar elliptic diurnal, second order.
	$\gamma-3\sigma-\varpi+2\eta$	Larger lunar evectional diurnal.
S_1 S_2 S_3 &c.	$\gamma-\eta$ $2(\gamma-\eta)$ $3(\gamma-\eta)$ &c.	Principal solar series.

Schedule of Notation—*continued.*

Initials.	Speed.	Name of Tide.
T	$2\gamma-3\eta$	Larger solar elliptic.
R	$2\gamma-\eta$	Smaller solar elliptic.
P	$\gamma-2\eta$	Solar diurnal.
Mm	$\sigma-\varpi$	Lunar monthly.
Mf	2σ	Lunar fortnightly.
Sa	η	Solar annual.
Ssa	2η	Solar semi-annual.
MSf	$2(\sigma-\eta)$	Luni-solar synodic fortnightly.
MS	$4\gamma-2\sigma-2\eta$	Compound tides.
μ or 2MS	$2\gamma-4\sigma+2\eta$	Compound tides.
2SM	$2\gamma+2\sigma-4\eta$	Compound tides.
MK	$3\gamma-2\sigma$	Compound tides.
2MK	$3\gamma-4\sigma$	Compound tides.
MN	$4\gamma-5\sigma+\varpi$	Compound tides.

The operations of the computers give the values of κ in degrees and two places of decimals of a degree, but as the values of κ are in no case so consistent from year to year as to present variations of less than a degree, the tables have been abridged by the entry merely of the nearest degree. The values of κ are printed in a different type from those of H, and the degree mark ° has been omitted.

In the case of the ports of Toulon and Brest the results in the Report of the Committee of the British Association were given in centimetres, but they have been reduced to feet for the sake of uniformity.

At the head of the table for each port the epoch, or instant, at which the analysed observations begin is noted; at every port (excepting

Kathiwadar) the epoch is 0h. of (old) astronomical time, or civil noon, of the day specified.

In Table I is given the latitude and longitude of the several ports.

In Table II the values are given of H and κ for each year or period analysed for the ports specified at the head; these are the values deduced from the results of 1872, 1876, 1878, and from those of the Indian Survey.

The initial of the tide is shown in the margin.

The last column for each port gives the mean of the values for the years under observation. An inspection of the numbers from which the mean is derived shows the degree of consistency between the numbers obtained in the several years. The number of results is hardly sufficient to make it worth while to deduce a probable error for H and κ; moreover, it would be a somewhat arduous task to do so.

Table III is a summary of Table II, giving only the mean values, together with the number of years from which the mean is derived, and this is of much value for the theoretical discussion of the tides.

Table IV gives Mr. Ferrel's results from the Reports to the United States Coast Survey.

The tables give altogether results for 43 ports, and for 137 periods of observation and analysis.

*[We have to thank Mr. Edward Roberts, the importance of whose work in this subject is well known, for having reduced the results given in the paper of 1878, viz., those for Freemantle, Mauritius, E. Falkland, Malta, Marseilles, and Toulon. In several of these the heights were stated in centimetres, but they are now reduced to feet and decimals.

Professor Ferrel has carried out an harmonic analysis at several ports for the United States Coast Survey. The process adopted by him does not appear to be identical with the method of the British Association, and there seemed to be room for doubt as to whether the results were truly comparable with ours. In answer to an inquiry on this point, addressed to the United States Coast Survey, Mr. Ferrel kindly sent a memorandum to the Superintendent, Mr. Hilgard, which has been forwarded for our information. The memorandum, dated Washington, April 27th, 1885, runs as follows:—

"The results of harmonic analyses of tide observations of the United States Coast and Geodetic Survey are found in Report of the British Association for 1872, and the Reports of the Coast and Geodetic Survey of 1878, App. No. 11; 1882, App. No. 17; 1883, App. No. 9. The results for Governor's Island have not yet been printed.

* This paragraph and the corresponding portion of the tables were added on May 15, 1885, subsequently to the presentation of the paper. These results of 1878 are only given in Table III, and not also in Table II.

"Those in the Report of the B.A. are by Sir W. Thomson. In those of the Coast and Geodetic Survey the A's (amplitude) correspond with Sir W. Thomson's R's, but the ϵ's (epochs) differ from his by 90° in the diurnal components. These corrections of his epochs I introduced into my 'Tidal Researches' in 1874, p. 44, § 28.*

"From a reference to Schedule I, Tides of Penobscot Bay, Professor Darwin with reason concludes that I have not applied this correction in the diurnal component of the κ-tides. This arises from the omission by oversight of a footnote to Schedule I, as follows:—

"'For λ^1 read $\lambda^1-\frac{1}{2}\pi$ in the diurnal component of the K-tide.'

"The corrections have, in all cases, been applied according to this note.

"In my 'Tidal Researches' of 1874 I have given formulæ for the correction of both the amplitudes and epochs depending upon the position of the moon's node. These corrections reduce them to what they would have been if the moon had moved in the ecliptic. By a reverse method these amplitudes and epochs can be reduced back to any year for which practical application of the results is required. In the discussion of tides in Penobscot Bay I have also given small tables, Tables III—VI inclusive, to facilitate these corrections, and reductions depending upon the lunar node. The double signs, however, of Tables III, V, and VI, got reversed somehow in copying and printing; but the signs have been used correctly in the reductions, even in those of the Report in which the signs are given erroneously, which shows that they were at first correct, and that the error was introduced in copying.

"These nodal corrections have in all cases been applied to the results, so that in these corrected results the irregularity of long period depending upon the moon's node is eliminated, and the amplitudes and epochs are the same from year to year, except so far as they are affected by small irregularities from abnormal disturbances not completely eliminated. An exception to this, however, is the case of the St. Thomas tides, in which the reductions were not carried so far, and these small nodal corrections were not applied to these small tides. The amplitudes and epochs are those simply belonging to the years of observations. . . . It is certainly desirable to have an international uniform notation.

"I should have stated sooner that in Table II, column C, 90° have

* [Notwithstanding this assurance I venture to think that Mr. Ferrel must be mistaken. For example, at Sandy Hook, it looks as though it were certain that K_2, L, λ have been reduced according to one rule, and the rest of the semi-diurnal tides according to another; for the phases differ by about 180°. Compare again O, K_1, P with J and Q at Penobscot Bay.—G. H. D., August 12, 1885.]

[It may be noticed that κ of S_1 for San Diego differs by 180° in the U.S. reduction from the value in the B.A. reduction. I have no evidence as to which is correct.—G. H. D., October, 1885.]

always been deducted before using it in the reductions in the case of the diurnal component of the K-tides."

We give below the results above referred to by Mr. Ferrel. We may remark, however, that the heights have been abridged by the omission of a place of decimals, and the epochs by the omission of the decimals of a degree. We have not, however, given quite all the results of the United States Coast Survey. Mr. Ferrel's treatment of M_1 is not identical with ours, and it is omitted; also there is no place vacant for some of the smaller overtides in our schedules. The reader especially interested in these tides is therefore advised to refer also to the original sources. The results for St. Thomas are derived from a letter dated March 10th, 1885, addressed by Mr. Ferrel to the superintendent, and kindly communicated to us.

From the correspondence it appears that the American results should be comparable with the others, or at least that the difference should be insignificant. As remarked, however, in a footnote on the preceding page, this conclusion is open to doubt. We have thought it best, therefore, to keep these results in a table separate from the others.]

Table I.

Indian Tide Tables.

	lat.			long.	
Aden	12°	47′ N.	....	44°	59′ E.
Karachi	24	47	....	66	58
Okha Point and Beyt Harbour, Gulf of Cutch	22	28	....	69	7
Kathiwadar or Shial Bet, S. coast of Kattywar	20	58	....	71	36
Bombay, Apollo Bunder	18	55	....	72	50
Karwar	14	48	....	74	6
Beypore, 7 miles S. of Calicut	11	10	....	75	49
Paumben Pass, island of Ramesweram	9	16	....	79	11
Negapatam	10	46	....	79	53
Madras	13	4	....	80	15
Vizagapatam	17	41	....	83	17
False Point	20	25	....	86	47
Dublat, Saugor Island, River Hooghly	21	38	....	88	6
Diamond Harbour, River Hooghly	22	11	....	88	14
Kidderpore, River Hooghly	22	32	....	88	22
Elephant Point	16	29	....	96	19
Rangoon	16	46	....	96	12
Amherst	16	5	....	97	34
Moulmein	16	29	....	97	40
Port Blair, Ross Island	11	41	....	92	45

British Association Reports.

N.B.—*Results for English ports are referred to Greenwich mean time.*

	lat.			long.	
Fort Point, California	37	40 N.	....	122	15 W.
San Diego, California	32	42	....	117	13
Port Leopold, Arctic Archipelago	74	—	....	91	—
Beechey Island, Erebus Bay, Arctic Archip.	74	43	....	91	54
Cat Island, Gulf of Mexico	30	23	....	89	0
Toulon	43	7	....	5	56 E.
Brest	48	23	....	4	30 W.
Ramsgate	51	18	....	1	21 E.
West Hartlepool	54	41	....	1	12 W.
Portland Breakwater	50	30	....	2	24
Liverpool	53	24	....	3	0
Helbre Island .... about	53	24	.. about	3	0
Freemantle, West Australia	32	3 S.	....	115	45 E.
Mauritius, Port Louis	20	9	....	57	11
East Falkland, Port Louis, Berkeley Sound	51	29	....	58	0 W.
Malta	35	55 N.	....	14	30 E.
Marseilles	43	18	....	5	23

United States Coast Survey Reports.

	lat.		long.
Penobscot Bay, Pulpit Cove, Maine.........	44 9 N.	....	68 53 W.
Port Townsend, Puget Sound, Washington Territory..............................	48 8	....	122 48
Astoria, Columbia River, Oregon...........	46 11	....	123 50
San Diego Bay, California.................	32 43	....	117 10
St. Thomas, West Indies..................	18 20	....	64 56
Sandy Hook..............................	40 27	....	74 1

Table II.

Aden.

Commence 0 h., March 3.

Year		1879–80.	1880–1.	1881–2.	1882–3.	Mean.
S_1	H =	0·073	0·117	0·093	0·077	0·090
	κ =	168	151	161	166	162
S_2	H =	0·693	0·693	0·704	0·699	0·697
	κ =	248	252	246	247	248
S_4	H =	0·006	0·005	0·006	0·005	0·006
	κ =	263	257	275	290	271
S_6	H =	0·005	0·004	0·004	0·004	0·004
	κ =	218	191	210	185	201
S_8	H =	0·001	0·001	0·001	0·001	0·001
	κ =	212	238	325	261	259
M_1	H =	0·033	0·052	0·053	0·048	0·047
	κ =	30	12	355	45	21
M_2	H =	1·578	1·558	1·569	1·567	1·568
	κ =	228	232	228	227	229
M_3	H =	0·019	0·020	0·018	0·016	0·018
	κ =	220	215	201	202	209
M_4	H =	0·011	0·006	0·007	0·003	0·007
	κ =	322	334	318	281	314
M_6	H =	0·006	0·004	0·004	0·007	0·005
	κ =	343	280	26	355	341
M_8	H =	0·003	0·001	0·004	0·002	0·003
	κ =	87	49	333	65	43
O	H =	0·657	0·658	0·646	0·651	0·653
	κ =	38	40	38	38	38
K_1	H =	1·295	1·297	1·299	1·305	1·299
	κ =	36	38	36	36	36
K_2	H =	0·218	0·197	0·188	0·202	0·201
	κ =	245	244	242	246	244
P	H =	0·389	0·375	0·389	0·399	0·388
	κ =	31	35	33	31	33
J	H =	0·118	0·110	0·083	0·100	0·103
	κ =	49	70	53	35	52

Table II.

Aden.

Commence 0 h., March 3.

Year		1879–80.	1880–1.	1881–2.	1882–3.	Mean.
Q	H =	0·174	0·157	0·134	0·139	0·151
	κ =	40	38	42	48	42
L	H =	0·023	0·063	0·033	0·065	0·046
	κ =	259	230	209	223	230
N	H =	0·443	0·436	0·421	0·409	0·427
	κ =	223	230	224	222	225
λ	H =	0·018	0·020	0·038	0·026	0·026
	κ =	123	301	210	155	197
ν	H =	0·157	0·132	0·059	0·048	0·099
	κ =	241	200	170	293	226
μ	H =	0·086	0·082	0·072	0·058	0·075
	κ =	192	204	182	204	196
R	H =	....	0·006	....	0·003	0·005
	κ =	....	64	....	356	30
T	H =	....	0·057	....	0·042	0·050
	κ =	....	286	....	194	240
MS	H =	0·007	0·020	0·009	0·011	0·012
	κ =	136	167	166	167	159
2SM	H =	0·022	0·021	0·021	0·026	0·023
	κ =	106	101	114	114	109
Mm	H =	0·035	0·076	0·025	0·033	0·042
	κ =	5	348	324	18	354
Mf	H =	0·052	0·039	0·045	0·044	0·045
	κ =	14	30	26	53	31
MSf	H =	0·014	0·015	0·016	0·010	0·014
	κ =	40	295	98	209	341
Sa	H =	0·404	0·402	0·353	0·399	0·390
	κ =	3	358	2	347	357
Ssa	H =	0·110	0·109	0·093	0·069	0·095
	κ =	94	161	151	99	126

Table II.

Karachi.

Commence 0 h., May 1.

Year		1868–9.	1869–70.	1870–1.	1871–2.	1872–3.	1873–4.
S_1	H =	0·072	0·071	0·075	0·083	0·108	0·083
	κ =	177	188	162	158	147	155
S_2	H =	0·932	0·943	0·923	0·951	0·952	0·943
	κ =	323	324	324	322	322	321
S_4	H =	......	......	0·014	0·013	0·008	0·010
	κ =	......	......	356	5	0	326
S_6	H =	......	......	......	0·004	0·012	0·004
	κ =	......	......	......	293	295	312
S_8	H =	......	......	......	......	......	0·000
	κ =	......	......	......	......	......	27
M_1	H =	0·013	......	0·030	0·063	0·040	0·038
	κ =	336	......	78	23	359	46
M_2	H =	2·511	2·447	2·450	2·492	2·476	2·471
	κ =	294	295	295	294	294	293
M_3	H =	0·042	0·037	0·048	0·048	0·037	0·030
	κ =	333	333	322	332	316	330
M_4	H =	0·016	0·027	0·024	0·029	0·020	0·022
	κ =	44	27	28	23	28	9
M_6	H =	0·040	0·046	0·044	0·045	0·046	0·048
	κ =	222	210	218	203	214	208
M_8	H =	......	......	......	0·006	0·006	0·003
	κ =	......	......	......	249	266	155
O	H =	0·662	0·645	0·629	0·636	0·632	0·645
	κ =	47	50	48	47	46	46
K_1	H =	1·278	1·257	1·255	1·279	1·275	1·269
	κ =	47	47	48	46	46	46
K_2	H =	0·299	0·273	0·260	0·293	0·292	0·274
	κ =	329	315	313	321	321	315
P	H =	0·376	0·385	0·375	0·360	0·368	0·393
	κ =	46	50	45	48	47	48
J	H =	0·091	0·046	0·070	0·107	0·104	0·059
	κ =	79	64	38	61	82	157

Table II.

Karachi.

Commence 0 h., May 1.

Year		1868–9.	1869–70.	1870–1.	1871–2.	1872–3.	1873–4.
Q	H =	0·129	0·120	0·138	0·146	0·129	0·119
	κ =	46	61	54	50	52	63
L	H =	0·079	0·047	0·089	0·043	0·137	0·084
	κ =	298	321	294	356	263	284
N	H =	0·604	0·587	0·572	0·650	0·605	0·587
	κ =	279	281	279	280	275	278
λ	H =	0·059	0·037	0·043	0·084	0·076	0·041
	κ =	335	270	209	207	12	259
ν	H =	0·190	0·081	0·080	0·143	0·191	0·116
	κ =	254	223	343	284	300	238
μ	H =	0·066	0·032	0·070	0·062	0·055	0·055
	κ =	267	224	300	254	270	232
R	H =	......	0·035	......	0·027	......	0·021
	κ =	......	271	......	221	......	228
T	H =	......	0·111	......	0·058	......	0·012
	κ =	......	320	......	22	......	233
MS	H =	0·017	0·024	0·031	0·020	......	0·023
	κ =	215	180	324	358	......	307
2SM	H =	......	......	......	......	......	0·007
	κ =	......	......	......	......	......	128
Mm	H =	0·069	0·040	0·031	......	......	0·055
	κ =	248	175	116	......	......	56
Mf	H =	0·053	0·078	0·037	......	......	0·012
	κ =	318	311	259	......	......	223
MSf	H =	0·009	0·074	0·057	......	......	0·042
	κ =	328	19	159	......	......	44
Sa	H =	0·115	0·179	0·162	......	......	0·250
	κ =	44	80	107	......	......	95
Ssa	H =	0·198	0·059	0·062	......	......	0·211
	κ =	82	117	70	......	.....	162

Table II.

Karachi.

Commence 0 h., May 1.

Year		1874–5.	1875–6.	1876–7.	1877–8.	1878–9.	1879–80.
S_1	H =	0·076	0·079	0·087	0·088	0·044	0·086
	κ =	153	150	157	181	167	161
S_2	H =	0·949	0·953	0·936	0·961	0·922	0·957
	κ =	320	320	318	321	324	325
S_4	H =	0·008	0·008	0·012	0·010	0·009	0·008
	κ =	6	353	17	23	29	63
S_6	H =	0·007	0·009	0·006	0·005	0·008	0·006
	κ =	309	295	259	275	291	325
S_8	H =	0·003	0·002	0·001	0·002	0·001	0·002
	κ =	266	283	207	254	126	223
M_1	H =	0·055	0·081	0·015	0·013	0·035	0·060
	κ =	66	36	353	76	54	14
M_2	H =	2·517	2·550	2·474	2·468	2·521	2·555
	κ =	292	291	291	291	296	296
M_3	H =	0·026	0·037	0·037	0·055	0·048	0·042
	κ =	336	345	343	327	328	320
M_4	H =	0·020	0·025	0·019	0·024	0·031	0·027
	κ =	8	15	353	16	2	7
M_6	H =	0·056	0·055	0·049	0·053	0·051	0·055
	κ =	212	206	207	196	215	220
M_8	H =	0·006	0·006	0·006	0·006	0·004	0·003
	κ =	196	297	281	252	269	20
O	H =	0·647	0·649	0·646	0·654	0·677	0·654
	κ =	46	46	45	46	49	47
K_1	H =	1·292	1·296	1·263	1·278	1·314	1·301
	κ =	46	46	44	45	48	48
K_2	H =	0·247	0·261	0·276	0·260	0·240	0·284
	κ =	316	321	318	314	329	325
P	H =	0·386	0·367	0·368	0·423	0·440	0·396
	κ =	46	45	49	43	44	45
J	H =	0·088	0·104	0·077	0·025	0·084	0·102
	κ =	35	50	71	88	66	64

Table II.

Karachi.

Commence 0 h., May 1.

Year	1874–5.	1875–6.	1876–7.	1877–8.	1878–9.	1879–80.
Q { H =	0·123	0·136	0·124	0·110	0·150	0·154
κ =	58	46	35	48	47	50
L { H =	0·088	0·042	0·085	0·039	0·054	0·066
κ =	280	302	306	305	263	312
N { H =	0·560	0·602	0·606	0·556	0·667	0·597
κ =	276	274	273	273	274	280
λ { H =	0·022	0·009	0·040	0·082	0·063	0·019
κ =	181	95	300	236	184	35
ν { H =	0·023	0·154	0·207	0·218	0·127	0·089
κ =	320	317	285	236	211	332
μ { H =	0·056	0·070	0·068	0·113	0·041	0·077
κ =	274	260	280	217	297	249
R { H =	......	......	0·008	......	0·069	......
κ =	......	......	308	......	273	......
T { H =	......	......	0·122	......	0·059	......
κ =	......	......	344	......	315	......
MS { H =	0·021	0·031	0·034	0·030	0·033	0·031
κ =	304	315	313	326	351	337
2SM { H =	0·018	0·025	0·012	0·026	0·019	0·018
κ =	150	98	115	63	167	158
Mm { H =	0·064	0·067	0·097	0·124	......	0·040
κ =	24	103	42	49	......	26
Mf { H =	0·038	0·010	0·032	0·047	......	0·030
κ =	41	24	1	30	......	328
MSf { H =	0·040	0·015	0·045	0·038	......	0·030
κ =	333	186	195	314	......	318
Sa { H =	0·149	0·086	0·197	0·170	......	0·042
κ =	56	76	80	120	......	86
Ssa { H =	0·172	0·173	0·145	0·087	......	0·165
κ =	157	167	164	70	......	171

Table II.

(*a*) *Karachi.* (*b*) *Okha.* (*c*) *Kathiwadar.*

(*a*) Com. 0h., May 1. (*b*) Com. 0 h., Apr. 16. (*c*) Com. 12 h., Oct. 31.

	(*a*)	(*a*)	(*a*)	(*a*)	(*b*)	(*c*)
Year	1880–1.	1881–2.	1882–3.	Mean.	1874–5.	1881–2.
S_1 H =	0·135	0·076	0·066	0·082	0·074	0·134
S_1 κ =	64	174	172	158	150	201
S_2 H =	0·969	0·960	0·962	0·948	1·222	1·207
S_2 κ =	324	324	324	322	14	81
S_4 H =	0·006	0·012	0·008	0·010	0·013	0·029
S_4 κ =	31	12	35	14	117	273
S_6 H =	0·006	0·008	0·006	0·007	0·003	0·013
S_6 κ =	292	314	287	295	21	42
S_8 H =	0·001	0·000	0·001	0·001	0·001	0·002
S_8 κ =	11	124	162	204	220	264
M_1 H =	0·059	0·048	0·062	0·044	0·051	0·057
M_1 κ =	319	39	61	30	43	35
M_2 H =	2·536	2·541	2·558	2·504	3·820	2·970
M_2 κ =	295	294	294	294	347	55
M_3 H =	0·039	0·034	0·030	0·039	0·030	0·020
M_3 κ =	319	327	336	330	21	152
M_4 H =	0·026	0·027	0·023	0·024	0·136	0·220
M_4 κ =	2	17	359	14	107	178
M_6 H =	0·052	0·049	0·044	0·049	0·007	0·139
M_6 κ =	209	207	204	210	270	137
M_8 H =	0·002	0·009	0·002	0·005	0·011	0·002
M_8 κ =	341	257	262	267	96	199
O H =	0·632	0·654	0·645	0·647	0·693	0·720
O κ =	46	46	47	47	57	66
K_1 H =	1·246	1·295	1·310	1·281	1·414	1·611
K_1 κ =	47	47	47	46	53	66
K_2 H =	0·415	0·269	0·234	0·278	0·328	0·324
K_2 κ =	322	317	321	320	17	79
P H =	0·266	0·396	0·396	0·380	0·384	0·436
P κ =	49	46	44	46	50	71
J H =	0·066	0·063	0·099	0·079	0·107	0·175
J κ =	106	53	38	70	81	107

Table II.

(*a*) *Karachi.* (*b*) *Okha.* (*c*) *Kathiwadar.*

(*a*) Com. 0. h., May 1. (*b*) Com. 0 h., April 16. (*c*) Com. 12 h., Oct. 31.

		(*a*)	(*a*)	(*a*)	(*a*)	(*b*)	(*c*)
Year ……		1880–1.	1881–2.	1882–3.	Mean.	1874–5.	1881–2.
Q	H =	0·104	0·121	0·132	0·129	0·137	0·152
	κ =	51	64	61	52	59	68
L	H =	0·123	0·096	0·076	0·081	0·221	0·079
	κ =	310	291	293	299	23	261
N	H =	0·581	0·631	0·594	0·600	0·781	0·755
	κ =	276	279	280	277	322	34
λ	H =	0·020	0·029	0·001	0·042	0·073	0·043
	κ =	313	275	236	282	23	107
ν	H =	0·169	0·211	0·125	0·142	0·164	0·131
	κ =	314	264	236	277	8	15
μ	H =	0·037	0·081	0·039	0·061	0·203	0·286
	κ =	283	267	270	263	182	343
R	H =	0·040	……	0·009	0·030		
	κ =	317	……	315	276		
T	H =	0·094	……	0·021	0·068		
	κ =	330	……	41	332		
MS	H =	0·028	0·030	0·024	0·027	0·064	0·159
	κ =	317	327	328	307	111	215
2SM	H =	0·031	0·030	0·021	0·021	0·044	0·029
	κ =	120	121	115	123	292	154
Mm	H =	0·036	0·055	0·040	0·060	0·066	0·052
	κ =	131	72	94	95	311	8
Mf	H =	0·020	0·034	0·006	0·033	0·050	0·027
	κ =	71	254	128	316	44	103
MSf	H =	0·018	0·043	0·023	0·036	0·141	0·040
	κ =	302	131	148	266	250	153
Sa	H =	0·102	0·100	0·099	0·138	0·162	0·236
	κ =	102	50	51	79	3	133
Ssa	H =	0·139	0·116	0·098	0·135	0·121	0·109
	κ =	192	164	194	142	145	156

Table II.

Bombay.

Commence 0 h., January 1.

Year	1878.	1879.	1880.	1881.	1882.	Mean.
S_1 H =	0·075	0·083	0·088	0·074	0·072	0·078
S_1 κ =	187	184	182	179	179	182
S_2 H =	1·614	1·634	1·627	1·618	1·616	1·622
S_2 κ =	3	2	4	3	4	3
S_4 H =	0·018	0·013	0·013	0·011	0·006	0·012
S_4 κ =	257	235	239	315	233	256
S_6 H =	0·002	0·004	0·004	0·005	0·002	0·003
S_6 κ =	195	179	140	160	182	171
S_8 H =	0·002	0·002	0·001	0·002	0·000	0·001
S_8 κ =	86	196	151	69	72	115
M_1 H =	0·024	0·036	0·086	0·065	0·045	0·051
M_1 κ =	46	105	51	19	26	49
M_2 H =	3·991	4·041	4·065	4·058	4·014	4·034
M_2 κ =	330	329	330	331	330	330
M_3 H =	0·074	0·067	0·068	0·055	0·060	0·065
M_3 κ =	32	28	25	11	21	23
M_4 H =	0·119	0·129	0·120	0·126	0·124	0·124
M_4 κ =	320	327	314	324	328	322
M_6 H =	0·014	0·015	0·002	0·017	0·008	0·011
M_6 κ =	130	110	79	113	124	111
M_8 H =	0·003	0·004	0·002	0·004	0·005	0·004
M_8 κ =	316	347	313	13	46	351
O H =	0·643	0·650	0·663	0·647	0·645	0·650
O κ =	48	48	48	48	49	48
K_1 H =	1·384	1·391	1·393	1·398	1·398	1·393
K_1 κ =	46	45	45	45	45	45
K_2 H =	0·412	0·394	0·427	0·431	0·388	0·410
K_2 κ =	349	353	355	353	351	352
P H =	0·404	0·400	0·406	0·403	0·396	0·402
P κ =	42	43	44	42	41	42
J H =	0·043	0·083	0·128	0·122	0·067	0·089
J κ =	89	48	62	88	74	72

Table II.

Bombay.

Commence 0 h., January 1.

Year		1878.	1879.	1880.	1881.	1882.	Mean.
Q	H =	0·122	0·138	0·159	0·183	0·101	0·131
	κ =	47	60	55	46	50	52
L	H =	0·122	0·054	0·128	0·094	0·143	0·108
	κ =	299	348	325	306	298	316
N	H =	1·024	1·036	0·991	0·974	0·988	1·003
	κ =	312	315	316	315	312	314
λ	H =	0·051	0·023	0·030	0·013	0·043	0·032
	κ =	284	254	157	203	277	235
ν	H =	0·288	0·245	0·078	0·121	0·261	0·199
	κ =	319	283	269	9	336	315
μ	H =	0·231	0·189	0·214	0·182	0·212	0·206
	κ =	313	294	314	301	318	308
R	H =	......	0·046	......	0·037	......	0·042
	κ =	......	301	......	265	......	283
T	H =	......	0·086	......	0·256	......	0·171
	κ =	......	46	......	2	......	24
MS	H =	0·122	0·138	0·126	0·125	0·134	0·129
	κ =	17	30	23	25	25	24
2SM	H =	0·039	0·025	0·048	0·036	0·033	0·036
	κ =	91	125	112	109	94	106
Mm	H =	0·058	0·049	0·042	0·047	0·085	0·056
	κ =	315	355	86	56	36	26
Mf	H =	0·068	0·054	0·054	0·029	0·052	0·051
	κ =	346	7	1	250	47	346
MSf	H =	0·056	0·016	0·042	0·019	0·023	0·031
	κ =	198	287	184	334	136	228
Sa	H =	0·254	0·137	0·173	0·188	0·179	0·186
	κ =	117	330	313	317	355	358
Ssa	H =	0·068	0·124	0·071	0·201	0·145	0·122
	κ =	304	223	162	232	218	228

Table II.

Karwar.

Commence 0 h., March 1.

Year		1878–9.	1879–80.	1880–1.	1881–2.	1882–3.	Mean.
S_1	H =	0·067	0·075	0·055	0·052	0·035	0·057
	κ =	159	149	156	165	167	159
S_2	H =	0·631	0·629	0·621	0·616	0·625	0·624
	κ =	335	336	334	333	335	335
S_4	H =	0·007	0·007	0·016	0·011	0·011	0·010
	κ =	115	87	94	110	92	100
S_6	H =	0·002	0·007	0·004	0·006	0·006	0·005
	κ =	32	58	82	51	39	52
S_8	H =	0·002	0·002	0·000	0·002	0·004	0·002
	κ =	344	295	297	283	303	304
M_1	H =	0·019	0·017	0·049	0·045	0·036	0·033
	κ =	70	45	29	10	48	41
M_2	H =	1·724	1·733	1·757	1·754	1·741	1·742
	κ =	303	303	301	301	301	302
M_3	H =	0·012	0·014	0·018	0·012	0·012	0·014
	κ =	280	286	275	264	261	273
M_4	H =	0·045	0·059	0·054	0·059	0·060	0·055
	κ =	28	22	11	16	7	17
M_6	H =	0·013	0·010	0·013	0·011	0·009	0·011
	κ =	289	283	277	284	287	284
M_8	H =	0·001	0·003	0·004	0·002	0·002	0·002
	κ =	210	51	9	58	215	109
O	H =	0·496	0·498	0·505	0·494	0·493	0·497
	κ =	50	50	49	48	49	49
K_1	H =	1·001	0·996	1·010	1·008	1·006	1·004
	κ =	47	47	45	44	45	45
K_2	H =	0·175	0·174	0·164	0·175	0·180	0·174
	κ =	330	329	327	333	330	330
P	H =	0·269	0·274	0·282	0·287	0·274	0·277
	κ =	41	43	43	41	40	42
J	H =	0·046	0·078	0·087	0·064	0·065	0·068
	κ =	51	55	71	67	42	57

Table II.

Karwar.

Commence 0 h., March 1.

Year ……		1878–9.	1879–80.	1880–1.	1881–2.	1882–3.	Mean.
Q	H =	0·111	0·133	0·130	0·101	0·097	0·114
	κ =	57	62	54	58	63	59
L	H =	0·093	0·041	0·059	0·038	0·050	0·056
	κ =	326	325	318	292	324	317
N	H =	0·416	0·426	0·413	0·400	0·397	0·410
	κ =	282	284	282	281	279	282
λ	H =	0·022	0·004	0·032	0·021	0·021	0·020
	κ =	244	122	29	341	268	273
ν	H =	0·077	0·136	0·122	0·057	0·047	0·088
	κ =	340	297	261	232	338	294
μ	H =	0·033	0·057	0·046	0·051	0·033	0·044
	κ =	283	245	260	244	284	263
R	H =	……	0·006	……	0·009	……	0·008
	κ =	……	61	……	230	……	145
T	H =	……	0·046	……	0·075	……	0·061
	κ =	……	9	……	300	……	155
MS	H =	0·022	0·028	0·021	0·029	0·028	0·026
	κ =	80	75	61	60	60	67
2SM	H =	0·012	0·004	0·004	0·007	0·009	0·007
	κ =	31	353	15	106	351	315
Mm	H =	0·046	0·061	0·048	0·043	0·126	0·065
	κ =	351	14	100	0	32	27
Mf	H =	0·051	0·058	0·034	0·038	0·027	0·042
	κ =	345	1	346	14	37	5
MSf	H =	0·029	0·023	0·021	0·009	0·030	0·022
	κ =	214	268	222	89	27	164
Sa	H =	0·170	0·344	0·491	0·383	0·373	0·352
	κ =	322	307	303	303	317	310
Ssa	H =	0·045	0·083	0·128	0·053	0·033	0·068
	κ =	297	202	191	224	225	228

Table II.

Beypore.

Commence 0 h., December 1.

Year ……	1878–9.	1879–80.	1880–1.	1881–2.	1882–3.	Mean.
S_1 { H =	0·021	0·083	0·093	0·073	0·035	0·061
κ =	120	207	187	185	173	174
S_2 { H =	0·331	0·310	0·308	0·341	0·359	0·330
κ =	20	19	22	17	12	18
S_4 { H =	0·004	0·003	0·004	0·006	0·007	0·005
κ =	140	118	133	145	148	137
S_6 { H =	0·004	0·004	0·003	0·006	0·010	0·005
κ =	252	244	266	227	248	247
S_8 { H =	0·001	0·000	0·001	0·001	0·001	0·001
κ =	21	252	45	319	339	339
M_1 { H =	0·017	0·032	0·038	0·024	0·032	0·029
κ =	146	69	23	40	90	73
M_2 { H =	0·907	0·904	0·895	0·950	1·001	0·931
κ =	330	330	333	329	324	329
M_3 { H =	0·011	0·010	0·011	0·010	0·009	0·010
κ =	214	184	200	196	194	197
M_4 { H =	0·021	0·015	0·018	0·020	0·026	0·020
κ =	45	36	53	41	31	41
M_6 { H =	0·010	0·004	0·003	0·006	0·012	0·007
κ =	121	114	184	138	133	138
M_8 { H =	0·008	0·010	0·007	0·008	0·009	0·008
κ =	137	130	140	162	162	146
O { H =	0·337	0·338	0·334	0·337	0·356	0·340
κ =	58	57	59	57	55	57
K_1 { H =	0·704	0·691	0·683	0·715	0·727	0·704
κ =	52	53	54	51	47	52
K_2 { H =	0·065	0·079	0·089	0·069	0·098	0·080
κ =	11	3	13	11	17	11
P { H =	0·184	0·188	0·197	0·177	0·211	0·191
κ =	49	56	57	54	48	53
J { H =	0·035	0·047	0·064	0·040	0·034	0·044
κ =	67	44	84	82	40	63

Table II.

Beypore.

Commence 0 h., December 1.

Year	1878-9.	1879-80.	1880-1.	1881-2.	1882-3.	Mean.
Q $\begin{cases} H = \\ \kappa = \end{cases}$	0·078 68	0·089 76	0·082 59	0·078 62	0·078 67	0·081 66
L $\begin{cases} H = \\ \kappa = \end{cases}$	0·018 349	0·037 341	0·020 342	0·033 347	0·025 1	0·027 348
N $\begin{cases} H = \\ \kappa = \end{cases}$	0·191 306	0·189 309	0·190 302	0·199 307	0·215 299	0·197 305
λ $\begin{cases} H = \\ \kappa = \end{cases}$	0·004 187	0·012 1	0·013 289	0·017 14	0·011 354	0·011 313
ν $\begin{cases} H = \\ \kappa = \end{cases}$	0·035 249	0·041 20	0·050 354	0·095 296	0·053 277	0·055 311
μ $\begin{cases} H = \\ \kappa = \end{cases}$	0·024 202	0·020 349	0·008 203	0·014 299	0·030 239	0·019 258
R $\begin{cases} H = \\ \kappa = \end{cases}$	....	0·017 163	....	0·028 101	....	0·023 132
T $\begin{cases} H = \\ \kappa = \end{cases}$	....	0·043 37	....	0·036 0	....	0·040 19
MS $\begin{cases} H = \\ \kappa = \end{cases}$	0·010 76	0·004 80	0·005 100	0·008 57	0·016 69	0·009 77
2SM $\begin{cases} H = \\ \kappa = \end{cases}$	0·006 65	0·004 219	0·004 241	0·004 243	0·007 350	0·005 296
Mm $\begin{cases} H = \\ \kappa = \end{cases}$	0·073 6	0·072 85	0·105 350	0·144 33	0·059 44	0·091 32
Mf $\begin{cases} H = \\ \kappa = \end{cases}$	0·086 15	0·086 18	0·022 50	0·118 48	0·044 346	0·071 23
Msf $\begin{cases} H = \\ \kappa = \end{cases}$	0·066 228	0·037 167	0·017 275	0·041 197	0·028 214	0·038 216
Sa $\begin{cases} H = \\ \kappa = \end{cases}$	0·307 311	0·344 316	0·328 311	0·321 329	0·243 298	0·309 313
Ssa $\begin{cases} H = \\ \kappa = \end{cases}$	0·139 226	0·252 181	0·180 208	0·189 193	0·123 214	0·177 205

Table II.

Paumben.

Commence 0 h., October 1.

Year	1878–9.	1879–80.	1880–1.	1881–2.	Mean.
S_1 { H =	0·036	0·049	0·035	0·022	0·036
κ =	146	131	153	163	148
S_2 { H =	0·377	0·375	0·377	0·360	0·372
κ =	90	92	91	94	92
S_4 { H =	0·005	0·001	0·004	0·003	0·003
κ =	287	191	262	304	261
S_6 { H =	0·002	0·001	0·005	0·006	0·004
κ =	246	168	195	179	197
S_8 { H =	0·004	0·005	0·002	0·001	0·003
κ =	249	255	257	135	224
M_1 { H =	0·013	0·009	0·013	0·008	0·011
κ =	17	38	64	19	35
M_2 { H =	0·589	0·585	0·598	0·569	0·585
κ =	47	47	46	49	47
M_3 { H =	0·016	0·016	0·015	0·017	0·016
κ =	170	168	165	177	170
M_4 { H =	0·020	0·016	0·015	0·014	0·016
κ =	199	190	199	187	194
M_6 { H =	0·011	0·011	0·011	0·009	0·011
κ =	42	50	40	34	42
M_8 { H =	0·005	0·004	0·004	0·007	0·005
κ =	294	348	303	313	314
O { H =	0·114	0·113	0·116	0·115	0·115
κ =	47	45	43	47	45
K_1 { H =	0·297	0·293	0·295	0·291	0·294
κ =	44	45	45	49	46
K_2 { H =	0·103	0·110	0·116	0·121	0·113
κ =	84	92	89	94	90
P { H =	0·105	0·110	0·108	0·115	0·110
κ =	44	47	46	50	46
J { H =	0·008	0·013	0·014	0·021	0·014
κ =	68	44	38	42	48

Table II.

Paumben.

Commence 0 h., October 1.

Year	1878–9.	1879–80.	1880–1.	1881–2.	Mean.
Q { H =	0·025	0·021	0·023	0·016	0·021
Q { κ =	84	98	91	81	89
L { H =	0·023	0·026	0·016	0·026	0·023
L { κ =	56	49	79	50	58
N { H =	0·076	0·087	0·084	0·082	0·082
N { κ =	29	30	32	32	31
λ { H =	0·017	0·023	0·008	0·014	0·016
λ { κ =	63	24	354	173	64
ν { H =	0·016	0·034	0·030	0·027	0·027
ν { κ =	82	49	15	334	30
μ { H =	0·004	0·010	0·012	0·011	0·009
μ { κ =	78	98	95	148	105
R { H =	....	0·012	....	0·019	0·016
R { κ =	....	133	....	94	114
T { H =	....	0·038	....	0·012	0·025
T { κ =	....	104	....	79	92
MS { H =	0·021	0·017	0·018	0·017	0·018
MS { κ =	292	294	286	295	292
2SM { H =	0·010	0·008	0·012	0·008	0·010
2SM { κ =	6	338	340	288	333
Mm { H =	0·063	0·053	0·033	0·043	0·048
Mm { κ =	349	58	23	40	27
Mf { H =	0·045	0·040	0·053	0·033	0·043
Mf { κ =	2	355	359	344	355
MSf { H =	0·016	0·013	0·027	0·007	0·016
MSf { κ =	174	209	157	27	141
Sa { H =	0·122	0·138	0·164	0·171	0·149
Sa { κ =	299	318	287	304	302
Ssa { H =	0·138	0·178	0·184	0·129	0·157
Ssa { κ =	96	110	117	111	108

Table II.

Negapatam. Commence 0 h., December 6.

Madras. Commence 0 h., February 1.

Year		1881–2.	1882–3.	Mean.	1880–1.	1881–2.	1882–3.	Mean.
S_1	H =	0·048	0·014	0·046	0·037	0·026	0·012	0·025
	κ =	117	100	108	80	96	99	92
S_2	H =	0·271	0·277	0·274	0·437	0·445	0·440	0·441
	κ =	283	286	284	277	275	276	276
S_4	H =	0·006	0·004	0·005	0·002	0·002	0·001	0·002
	κ =	136	166	151	98	169	218	161
S_6	H =	0·000	0·000	0·000	0·002	0·001	0·001	0·001
	κ =	135	166	150	61	99	176	112
S_8	H =	0·001	0·001	0·001	0·001	0·000	0·001	0·001
	κ =	225	228	227	131	63	291	162
M_1	H =	0·003	0·006	0·005	0·019	0·001	0·004	0·008
	κ =	149	73	111	4	312	65	7
M_2	H =	0·712	0·727	0·720	1·047	1·051	1·049	1·049
	κ =	251	252	252	249	247	248	248
M_3	H =	0·003	0·003	0·003	0·004	0·003	0·006	0·004
	κ =	73	133	103	65	55	67	62
M_4	H =	0·023	0·018	0·021	0·002	0·001	0·005	0·003
	κ =	76	77	77	130	115	193	146
M_6	H =	0·010	0·013	0·012	0·010	0·011	0·009	0·010
	κ =	130	126	128	161	149	154	154
M_8	H =	0·005	0·004	0·005	0·002	0·001	0·002	0·002
	κ =	297	309	303	331	84	83	46
O	H =	0·092	0·089	0·091	0·094	0·096	0·101	0·097
	κ =	320	323	322	327	324	325	325
K_1	H =	0·222	0·227	0·225	0·294	0·291	0·293	0·293
	κ =	345	345	345	340	338	342	340
K_2	H =	0·071	0·082	0·077	0·121	0·120	0·094	0·112
	κ =	281	291	286	278	276	286	280
P	H =	0·083	0·085	0·084	0·093	0·094	0·103	0·097
	κ =	342	350	346	341	341	350	344
J	H =	0·006	0·016	0·011	0·029	0·012	0·021	0·021
	κ =	348	307	328	337	314	304	318

Table II.

	Negapatam. Commence 0 h., December 6.			*Madras.* Commence 0 h., February 1.			
Year	1881–2.	1882–3.	Mean.	1880–1.	1881–2.	1882–3.	Mean.
Q { H =	0·007	0·007	0·007	0·004	0·003	0·009	0·005
Q { κ =	143	219	181	140	150	43	111
L { H =	0·022	0·031	0·027	0·037	0·017	0·054	0·036
L { κ =	278	279	279	278	335	310	307
N { H =	0·164	0·152	0·158	0·246	0·235	0·238	0·240
N { κ =	242	246	244	243	240	242	242
λ { H =	0·025	0·005	0·015	0·027	0·025	0·035	0·029
λ { κ =	234	229	231	348	283	268	299
ν { H =	0·048	0·047	0·048	0·053	0·007	0·072	0·044
ν { κ =	228	206	217	209	287	318	271
μ { H =	0·018	0·024	0·021	0·046	0·048	0·030	0·041
μ { κ =	132	113	122	184	167	183	178
R { H =	......	0·031	0·031	......	0·016	......	0·016
R { κ =	......	349	349	......	103	......	103
T { H =	......	0·050	0·050	......	0·056	......	0·056
T { κ =	......	255	255	......	257	......	257
MS { H =	0·019	0·017	0·018	0·001	0·001	0·004	0·003
MS { κ =	96	96	96	177	54	280	170
2MS { H =	0·007	0·006	0·007	0·020	0·022	0·023	0·022
2MS { κ =	161	216	188	228	220	178	209
Mm { H =	0·081	0·032	0·057	0·040	0·047	0·055	0·047
Mm { κ =	345	310	328	41	130	68	80
Mf { H =	0·061	0·017	0·039	0·030	0·050	0·055	0·045
Mf { κ =	35	338	7	5	349	25	6
MSf { H =	0·084	0·097	0·091	0·001	0·034	0·021	0·019
MSf { κ =	2	13	7	84	46	44	58
Sa { H =	0·543	0·522	0·533	0·372	0·335	0·449	0·385
Sa { κ =	231	233	232	201	225	211	212
Ssa { H =	0·400	0·316	0·358	0·275	0·383	0·257	0·305
Ssa { κ =	126	134	130	120	149	115	128

Table II.

Vizagapatam.

Commence 0 h., February 3.

Year		1879–80.	1880–1.	1881–2.	1882-3.	Mean.
S_1	H =	0·028	0·047	0·035	0·096	0·052
	κ =	101	77	92	1	68
S_2	H =	0·674	0·659	0·651	0·641	0·656
	κ =	280	286	286	290	285
S_4	H =	0·001	0·007	0·006	0·006	0·005
	κ =	0	77	50	60	47
S_6	H =	0·001	0·001	0·001	0·002	0·001
	κ =	124	128	214	215	170
S_8	H =	0·001	0·001	0·003	0·001	0·002
	κ =	73	103	61	78	79
M_1	H =	0·023	0·021	0·001	0·004	0·012
	κ =	355	23	199	242	295
M_2	H =	1·532	1·460	1·459	1·439	1·473
	κ =	249	253	254	255	253
M_3	H =	0·007	0·001	0·006	0·008	0·006
	κ =	332	208	41	14	329
M_4	H =	0·014	0·014	0·015	0·018	0·015
	κ =	310	331	339	342	331
M_6	H =	0·003	0·004	0·005	0·008	0·005
	κ =	144	78	30	35	72
M_8	H =	0·004	0·002	0·002	0·005	0·003
	κ =	174	214	243	206	209
O	H =	0·139	0·140	0·144	0·142	0·141
	κ =	330	332	333	329	331
K_1	H =	0·371	0·364	0·366	0·335	0·359
	κ =	338	342	342	346	342
K_2	H =	0·179	0·157	0·168	0·306	0·203
	κ =	270	274	285	278	277
P	H =	0·112	0·104	0·117	0·049	0·096
	κ =	336	346	346	329	339
J	H =	0·035	0·027	0·014	0·024	0·025
	κ =	328	356	314	351	337

Table II.

Vizagapatam.

Commence 0 h., February 3.

Year		1879–80.	1880–1.	1881–2.	1882–3.	Mean.
Q	H =	0·010	0·007	0·004	0·014	0·009
	κ =	20	277	306	336	325
L	H =	0·049	0·044	0·027	0·088	0·052
	κ =	257	245	297	217	254
N	H =	0·355	0·300	0·291	0·309	0·314
	κ =	243	250	251	242	246
λ	H =	0·021	0·019	0·022	0·024	0·022
	κ =	201	332	244	278	264
ν	H =	0·114	0·055	0·002	0·127	0·075
	κ =	244	199	72	283	199
μ	H =	·0·030	0·026	0·016	0·034	0·027
	κ =	234	259	218	326	259
R	H =	......	0·015	......	0·039	0·027
	κ =	......	130	......	246	188
T	H =	......	0·021	......	0·080	0·051
	κ =	......	336	......	189	263
MS	H =	0·007	0·010	0·014	0·015	0·012
	κ =	345	20	20	357	5
2MS	H =	0·008	0·010	0·015	0·016	0·012
	κ =	210	292	250	148	225
Mm	H =	0·022	0·078	0·049	0·072	0·055
	κ =	22	53	104	35	54
Mf	H =	0·030	0·051	0·061	0·027	0·042
	κ =	23	340	2	2	2
MSf	H =	0·076	0·021	0·038	0·048	0·046
	κ =	22	13	314	102	23
Sa	H =	0·740	0·833	0·577	0·707	0·714
	κ =	190	173	189	175	182
Ssa	H =	0·301	0·328	0·458	0·241	0·332
	κ =	89	126	140	101	114

Table II.

	False Point. Commence 0 h., May 1.			*Dublat.* Commence 0 h., April 22.		
Year	1881–2.	1882–3.	Mean.	1881–2.	1882–3.	Mean.
S_1 {H =	0·006	0·024	0·015	0·044	0·050	0·047
κ =	325	48	6	99	121	110
S_2 {H =	1·005	1·030	1·018	2·053	2·163	2·108
κ =	302	304	303	327	326	327
S_4 {H =	0·007	0·008	0·008	0·025	0·011	0·018
κ =	331	327	329	202	220	211
S_6 {H =	0·003	0·003	0·003	0·002	0·005	0·004
κ =	153	185	169	120	78	99
S_8 {H =	0·003	0·003	0·003	0·004	0·007	0·006
κ =	219	261	240	116	110	113
M_1 {H =	0·009	0·008	0·009	0·008	0·007	0·008
κ =	66	355	30	345	97	41
M_2 {H =	2·247	2·253	2·250	4·623	4·596	4·610
κ =	269	271	270	290	290	290
M_3 {H =	0·012	0·016	0·014	0·049	0·043	0·046
κ =	34	27	30	131	135	133
M_4 {H =	0·035	0·041	0·038	0·101	0·089	0·095
κ =	224	236	230	143	145	144
M_6 {H =	0·006	0·014	0·010	0·014	0·013	0·014
κ =	80	47	63	275	236	255
M_8 {H =	0·003	0·002	0·003	0·014	0·009	0·012
κ =	229	262	246	316	273	294
O {H =	0·175	0·179	0·177	0·181	0·197	0·189
κ =	335	335	335	332	335	334
K_1 {H =	0·408	0·407	0·408	0·498	0·488	0·493
κ =	344	346	345	353	350	352
K_2 {H =	0·268	0·241	0·255	0·573	0·618	0·596
κ =	296	297	297	310	325	318
P {H =	0·133	0·157	0·145	0·158	0·151	0·155
κ =	349	340	344	336	351	343
J {H =	0·021	0·030	0·026	0·031	0·016	0·024
κ =	306	319	312	352	296	324

Table II.

False Point. Commence 0 h., May 1.

Dublat. Commence 0 h., April 22.

Year		1881–2.	1882–3.	Mean.	1881–2.	1882–3.	Mean.
Q	H =	0·004	0·017	0·011	0·010	0·008	0·009
	κ =	307	340	324	306	1	333
L	H =	0·068	0·050	0·059	0·175	0·158	0·167
	κ =	281	227	254	291	292	291
N	H =	0·471	0·481	0·476	1·041	0·852	0·947
	κ =	265	268	267	285	286	286
λ	H =	0·045	0·081	0·063	0·298	0·139	0·219
	κ =	277	83	180	339	293	316
ν	H =	0·163	0·120	0·142	0·271	0·192	0·232
	κ =	247	241	244	261	240	251
μ	H =	0·070	0·080	0·075	0·218	0·111	0·165
	κ =	266	280	273	10	19	15
R	H =	......	0·034	0·034	......	0·219	0·219
	κ =	......	217	217	......	289	289
T	H =	......	0·017	0·017	......	0·137	0·137
	κ =	......	149	149	......	299	299
MS	H =	0·039	0·042	0·041	0·094	0·059	0·077
	κ =	272	275	274	171	139	155
2MS	H =	0·019	0·014	0·017	0·097	0·046	0·072
	κ =	196	177	187	195	227	211
Mm	H =	0·053	0·072	0·063	0·045	0·035	0·040
	κ =	53	58	55	29	125	77
Mf	H =	0·061	0·073	0·067	0·056	0·039	0·048
	κ =	37	33	35	61	71	66
MSf	H =	0·041	0·059	0·050	0·049	0·077	0·063
	κ =	279	73	356	278	75	356
Sa	H =	0·746	0·840	0·793	0·796	1·003	0·900
	κ =	166	166	166	147	154	150
Ssa	H =	0·364	0·210	0·287	0·234	0·182	0·208
	κ =	142	149	146	162	110	136

Table II.

	Diamond Harbour. Commence 0 h., April 4.			*Kidderpore.* Commence 0 h., March 22.		
Year	1881–2.	1882–3.	Mean.	1881–2.	1882–3.	Mean.
S_1 { H =	0·082	0·088	0·085	0·094	0·088	0·091
S_1 { κ =	156	147	152	197	190	193
S_2 { H =	2·215	2·288	2·252	1·427	1·508	1·468
S_2 { κ =	26	25	26	102	101	101
S_4 { H =	0·117	0·122	0·120	0·066	0·084	0·075
S_4 { κ =	328	323	326	126	111	119
S_6 { H =	0·013	0·013	0·013	0·006	0·004	0·005
S_6 { κ =	266	235	251	266	332	299
S_8 { H =	0·002	0·004	0·003	0·006	0·009	0·008
S_8 { κ =	305	42	353	298	323	311
M_1 { H =	0·020	0·020	0·020	0·012	0·013	0·013
M_1 { κ =	88	103	95	112	202	157
M_2 { H =	5·175	5·179	5·177	3·593	3·660	3·627
M_2 { κ =	345	344	344	59	58	58
M_3 { H =	0·042	0·028	0·035	0·012	0·018	0·015
M_3 { κ =	220	225	223	335	328	331
M_4 { H =	0·756	0·734	0·745	0·734	0·719	0·727
M_4 { κ =	246	245	246	39	35	37
M_6 { H =	0·156	0·148	0·152	0·158	0·160	0·159
M_6 { κ =	106	105	106	323	315	319
M_8 { H =	0·065	0·058	0·062	0·074	0·082	0·078
M_8 { κ =	347	343	345	276	263	270
O { H =	0·237	0·230	0·234	0·228	0·211	0·220
O { κ =	344	346	345	22	20	21
K_1 { H =	0·499	0·492	0·496	0·390	0·387	0·389
K_1 { κ =	15	14	14	58	54	56
K_2 { H =	0·667	0·644	0·656	0·439	0·431	0·435
K_2 { κ =	20	27	23	90	101	96
P { H =	0·176	0·174	0·175	0·146	0·142	0·144
P { κ =	6	12	9	42	52	47
J { H =	0·029	0·033	0·031	0·016	0·012	0·014
J { κ =	299	340	320	355	298	327

Table II.

	Diamond Harbour. Commence 0 h., April 4.			*Kidderpore.* Commence 0 h., March 22.		
Year	1881–2.	1882–3.	Mean.	1881–2.	1882–3.	Mean.
Q { H =	0·024	0·036	0·030	0·039	0·039	0·039
{ κ =	9	10	9	358	20	9
L { H =	0·174	0·347	0·261	0·201	0·173	0·187
{ κ =	357	344	351	86	62	74
N { H =	0·988	0·914	0·951	0·677	0·599	0·638
{ κ =	339	340	339	48	46	47
λ { H =	0·171	0·058	0·115	0·126	0·075	0·101
{ κ =	19	296	337	131	84	107
ν { H =	0·420	0·186	0·303	0·323	0·152	0·238
{ κ =	294	284	289	358	349	353
μ { H =	0·272	0·333	0·303	0·224	0·260	0·242
{ κ =	79	90	85	174	190	182
R { H =	......	0·216	0·216	......	0·167	0·167
{ κ =	......	10	10	......	77	77
T { H =	......	0·078	0·078	......	0·147	0·147
{ κ =	......	55	55	......	107	107
MS { H =	0·687	0·702	0·695	0·646	0·643	0·645
{ κ =	286	284	285	82	80	81
2SM { H =	0·095	0·053	0·074	0·084	0·086	0·085
{ κ =	251	290	271	355	9	2
Mm { H =	0·147	0·057	0·102	0·316	0·172	0·244
{ κ =	12	351	1	0	341	351
Mf { H =	0·157	0·142	0·150	0·301	0·293	0·297
{ κ =	36	41	39	41	36	38
MSf { H =	0·401	0·501	0·451	0·829	0·920	0·875
{ κ =	26	40	33	35	43	39
Sa { H =	1·011	1·189	1·100	2·809	2·670	2·740
{ κ =	140	147	143	157	157	157
Ssa { H =	0·023	0·109	0·066	0·935	0·708	0·822
{ κ =	64	77	71	205	334	269

Table II.

(*a*) *Elephant Point.* *Rangoon.*

(*a*) Commence 0 h., May 24. Commence 0 h., March 1.

Year	(*a*) 1880–1.	1880–1.	1881–2.	1882–3.	Mean.
S_1 H =	0·113	0·120	0·123	0·097	0·113
S_1 κ =	79	141	129	129	133
S_2 H =	2·337	2·009	2·003	2·025	2·012
S_2 κ =	143	169	170	171	170
S_4 H =	0·037	0·076	0·088	0·079	0·081
S_4 κ =	162	262	256	258	259
S_6 H =	0·021	0·011	0·009	0·011	0·010
S_6 κ =	94	42	39	63	48
S_8 H =	0·008	0·006	0·003	0·005	0·005
S_8 κ =	60	119	117	122	120
M_1 H =	0·019	0·049	0·037	0·013	0·033
M_1 κ =	88	151	236	163	183
M_2 H =	5·870	5·539	5·519	5·577	5·545
M_2 κ =	103	130	132	131	131
M_3 H =	0·025	0·009	0·016	0·038	0·021
M_3 κ =	146	238	154	142	178
M_4 H =	0·079	0·388	0·424	0·418	0·410
M_4 κ =	46	167	171	168	169
M_6 H =	0·205	0·236	0·227	0·235	0·233
M_6 κ =	349	85	89	87	87
M_8 H =	0·031	0·074	0·083	0·087	0·081
M_8 κ =	322	92	103	96	97
O H =	0·349	0·289	0·294	0·300	0·294
O κ =	356	30	27	28	28
K_1 H =	0·807	0·674	0·682	0·653	0·670
K_1 κ =	18	35	35	36	35
K_2 H =	0·401	0·535	0·576	0·598	0·570
K_2 κ =	91	168	173	165	169
P H =	0·199	0·134	0·148	0·166	0·149
P κ =	33	61	52	53	55
J H =	0·110	0·049	0·023	0·018	0·030
J κ =	61	70	91	298	33

Table II.

(*a*) *Elephant Point.* *Rangoon.*

(*a*) Commence 0 h., May 24. Commence 0 h., March 1.

(*a*)

Year	1880–1.	1880–1.	1881–2.	1882–3.	Mean.
Q $\{H =$	0·042	0·028	0·024	0·028	0·027
$\kappa =$	336	9	29	56	31
L $\{H =$	0·346	0·368	0·327	0·525	0·407
$\kappa =$	109	153	158	160	157
N $\{H =$	1·543	1·045	0·949	0·977	0·990
$\kappa =$	80	117	120	115	117
λ $\{H =$	0·659	0·299	0·290	0·181	0·257
$\kappa =$	145	174	184	152	170
ν $\{H =$	0·681	0·479	0·288	0·184	0·317
$\kappa =$	209	94	75	130	100
μ $\{H =$	0·356	0·497	0·508	0·536	0·514
$\kappa =$	279	289	295	286	290
R $\{H =$	....	....	0·117	....	0·117
$\kappa =$	....	....	66	....	66
T $\{H =$	....	....	0·290	....	0·290
$\kappa =$	....	....	128	....	128
MS $\{H =$	0·135	0·349	0·415	0·394	0·386
$\kappa =$	67	207	212	210	210
2SM $\{H =$	0·042	0·173	0·155	0·153	0·160
$\kappa =$	84	46	54	61	54
Mm $\{H =$	0·145	0·296	0·230	0·182	0·236
$\kappa =$	6	21	9	39	23
Mf $\{H =$	0·098	0·168	0·223	0·233	0·208
$\kappa =$	310	35	27	39	34
MSf $\{H =$	0·059	0·515	0·559	0·588	0·554
$\kappa =$	273	45	52	49	49
Sa $\{H =$	0·930	1·600	1·415	1·444	1·486
$\kappa =$	146	144	153	152	150
Ssa $\{H =$	0·261	0·193	0·012	0·174	0·126
$\kappa =$	198	306	315	3	328

Table II.

Amherst. Commence 0 h., August 5. — *Moulmein.* Commence 0 h., April 17.

N.B.—The MS. gives H of $K_2 = 1 \cdot 771$ for 1880–1; an obvious mistake. The mean has been corrected.

Year		Amherst 1880–1.	Amherst 1881–2.	Amherst 1882–3.	Amherst Mean.	Moulmein 1880–1.	Moulmein 1881–2.	Moulmein 1882–3.	Moulmein Mean.
S_1	H =	0·426	0·143	0·096	0·222	0·095	0·099	0·095	0·096
	κ =	178	149	97	141	146	155	148	149
S_2	H =	2·851	2·705	2·750	2·769	1·400	1·344	1·343	1·362
	κ =	109	101	105	105	148	145	150	148
S_4	H =	0·095	0·118	0·104	0·106	0·068	0·069	0·065	0·067
	κ =	147	102	117	122	230	225	231	229
S_6	H =	0·022	0·004	0·009	0·012	0·006	0·006	0·004	0·005
	κ =	222	221	119	187	178	187	185	183
S_8	H =	0·009	0·006	0·009	0·008	0·002	0·001	0·002	0·002
	κ =	209	348	269	276	164	252	218	211
M_1	H =	0·041	0·021	0·035	0·032	0·034	0·019	0·002	0·018
	κ =	192	273	300	255	128	101	183	138
M_2	H =	6·230	6·081	6·389	6·233	3·884	3·698	3·756	3·779
	κ =	70	68	68	69	112	112	115	113
M_3	H =	0·034	0·003	0·019	0·019	0·023	0·031	0·020	0·025
	κ =	287	224	273	261	274	215	139	209
M_4	H =	0·273	0·423	0·355	0·350	0·926	0·880	0·897	0·901
	κ =	60	51	41	51	169	170	174	171
M_6	H =	0·070	0·146	0·139	0·118	0·105	0·107	0·095	0·102
	κ =	257	248	251	252	200	198	201	200
M_8	H =	0·006	0·014	0·021	0·014	0·034	0·036	0·044	0·038
	κ =	282	244	220	249	125	141	134	133
O	H =	0·310	0·319	0·323	0·317	0·256	0·252	0·252	0·253
	κ =	328	339	352	339	44	49	50	48
K_1	H =	0·668	0·686	0·744	0·699	0·452	0·447	0·414	0·438
	κ =	3	6	6	5	39	40	42	40
K_2	H =	0·771	0·858	0·682	0·770	0·409	0·282	0·316	0·336
	κ =	91	81	97	90	151	152	162	155
P	H =	0·132	0·193	0·207	0·177	0·113	0·144	0·144	0·134
	κ =	308	348	354	337	62	61	57	60
J	H =	0·109	0·083	0·031	0·074	0·038	0·018	0·009	0·022
	κ =	13	7	82	34	52	48	222	107

Table II.

	Amherst. Commence 0 h., August 5.				*Moulmein.* Commence 0 h., April 17.			
Year	1880–1.	1881–2.	1882–3.	Mean.	1880–1.	1881–2.	1882–3.	Mean.
Q { H =	0·064	0·060	0·039	0·054	0·043	0·054	0·039	0·045
κ =	325	321	322	322	53	55	53	53
L { H =	0·226	0·303	0·348	0·292	0·204	0·390	0·242	0·279
κ =	112	120	103	112	134	155	129	139
N { H =	1·374	1·248	1·343	1·322	0·735	0·672	0·630	0·679
κ =	60	51	51	54	97	106	102	102
λ { H =	0·393	0·280	0·226	0·300	0·161	0·249	0·118	0·176
κ =	113	65	178	119	152	182	162	165
ν { H =	0·426	0·283	0·566	0·425	0·314	0·215	0·169	0·233
κ =	186	267	79	177	101	91	58	84
μ { H =	0·443	0·247	0·220	0·303	0·308	0·314	0·316	0·313
κ =	278	299	326	301	272	259	280	270
R { H =	......	0·451	......	0·451	......	0·097	......	0·097
κ =	......	252	......	252	......	70	......	70
T { H =	......	0·841	......	0·841	......	0·200	......	0·200
κ =	......	144	......	144	......	110	......	110
MS { H =	0·285	0·406	0·350	0·347	0·741	0·701	0·693	0·712
κ =	90	80	76	82	210	209	214	211
2SM { H =	0·188	0·150	0·115	0·151	0·127	0·137	0·109	0·124
κ =	345	28	16	10	38	40	37	38
Mm { H =	......	0·152	0·038	0·095	0·409	0·441	0·229	0·360
κ =	......	43	52	48	3	17	21	14
Mf { H =	......	0·062	0·132	0·097	0·282	0·379	0·342	0·334
κ =	......	315	24	350	42	40	40	41
Msf { H =	......	0·080	0·029	0·055	1·088	1·097	1·146	1·110
κ =	......	76	66	71	43	48	46	46
Sa { H =	......	0·638	0·814	0·726	2·460	2·389	2·453	2·434
κ =	......	150	130	140	145	153	149	149
Saa { H =	......	0·188	0·124	0·156	0·563	0·653	0·593	0·603
κ =	......	139	332	235	283	284	295	287

Table II.

Port Blair. Commence 0 h., April 19.

Fort Point, California. Commence 0 h., October 1.

Year		1880–1.	1881–2.	1882–3.	Mean.	1858–9.	1859–60.	1860–1.	Mean.
S_1	H =	0·028	0·018	0·016	0·021	0·015	......	......	0·015
	κ =	49	35	31	38	212	......	......	212
S_2	H =	0·966	0·978	0·959	0·968	0·407	0·380	0·382	0·390
	κ =	316	313	315	315	334	336	336	336
S_4	H =	0·003	0·001	0·004	0·003				
	κ =	107	86	59	84				
S_6	H =	0·002	0·002	0·002	0·002				
	κ =	152	99	142	131				
S_8	H =	0·002	0·002	0·001	0·002				
	κ =	98	88	53	80				
M_1	H =	0·016	0·007	0·008	0·010	0·034	0·037	0·044	0·038
	κ =	23	254	238	291	98	273	139	170
M_2	H =	2·042	2·014	2·010	2·022	1·722	1·659	1·685	1·689
	κ =	279	277	278	278	332	333	331	332
M_3	H =	0·004	0·011	0·007	0·007				
	κ =	20	11	16	16				
M_4	H =	0·003	0·011	0·011	0·008	0·066	0·074	0·072	0·071
	κ =	167	128	158	151	26	30	15	24
M_6	H =	0·004	0·002	0·000	0·002				
	κ =	342	206	42	317				
M_8	H =	0·003	0·002	0·002	0·002				
	κ =	19	70	120	70				
O	H =	0·153	0·162	0·166	0·160	0·769	0·756	0·814	0·780
	κ =	299	304	302	302	89	89	85	87
K_1	H =	0·403	0·397	0·391	0·397	1·217	1·209	1·232	1·219
	κ =	326	327	327	327	106	107	107	107
K_2	H =	0·286	0·296	0·264	0·282	0·139	0·143	0·122	0·135
	κ =	314	308	310	311	336	328	325	330
P	H =	0·130	0·137	0·134	0·134	0·367	0·366	0·387	0·373
	κ =	324	327	326	326	107	106	104	105
J	H =	0·038	0·030	0·014	0·027	0·072	0·034	0·053	0·053
	κ =	316	324	333	325	130	127	105	121

Table II.

Port Blair. Commence 0 h., April 19.

Fort Point, California. Commence 0 h., October 1.

Year		1880–1.	1881–2.	1882–3.	Mean.	1858–9.	1859–60.	1860–1.	Mean.
Q	H =	0·023	0·027	0·023	0·024	0·147	0·094	0·123	0·121
	κ =	236	242	233	237	78	54	90	74
L	H =	0·059	0·098	0·046	0·068	0·053	0·060	0·064	0·059
	κ =	269	290	258	272	300	18	335	338
N	H =	0·413	0·392	0·391	0·399	0·406	0·357	0·359	0·374
	κ =	273	273	277	274	305	307	305	305
λ	H =	0·035	0·046	0·047	0·043	0·038	0·029	0·012	0·026
	κ =	229	311	301	280	9	338	326	345
ν	H =	0·148	0·137	0·079	0·121	0·107	0·040	0·045	0·064
	κ =	294	254	214	254	288	274	352	305
μ	H =	0·094	0·089	0·074	0·086	0·028	0·032	0·026	0·029
	κ =	288	298	291	292	257	210	214	227
R	H =	......	0·020	......	0·020	......	0·008	......	0·008
	κ =	......	326	......	326	......	63	......	63
T	H =	......	0·099	......	0·099	......	0·014	......	0·014
	κ =	......	313	......	313	......	198	......	198
MS	H =	0·004	0·016	0·007	0·009	0·026	0·034	0·032	0·031
	κ =	153	206	284	215	23	14	25	21
2SM	H =	0·021	0·020	0·028	0·023				
	κ =	149	168	146	154				
Mm	H =	0·020	0·017	0·005	0·014				
	κ =	13	26	21	20				
Mf	H =	0·056	0·067	0·048	0·057				
	κ =	356	15	17	9				
MSf	H =	0·019	0·007	0·018	0·015				
	κ =	168	4	9	61				
Sa	H =	0·299	0·062	0·251	0·204				
	κ =	163	133	156	150				
Ssa	H =	0·106	0·134	0·110	0·117				
	κ =	165	197	170	177				

Table II.

(*a*) *San Diego.* (*b*) *Port Leopold.* (*c*) *Beechey Island.* (*d*) *Cat Island.* (*e*) *Toulon.*

(*a*) Com. 0 h., Jan. 1. (*b*) Com. 0 h., Nov. 1, 1848, to July 31, 1849. (*c*) Com. 0 h., Nov. 2, 1858, to Feb. 28, 1859. (*d*) Com. 0 h., Jan. 1. (*e*) Com. 0 h., Jan. 1.

	(*a*)	(*a*)	(*a*)	(*b*)	(*c*)	(*d*)	(*e*)
Year	1860.	1861.	Mean.	1848-9.	1858-9.	1848.	1853.
S_1 { H =	0·030	0·025	0·028	0·031	......	0·044	0·010
S_1 { κ =	229	246	238	27	......	10	186
S_2 { H =	0·697	0·693	0·695	0·643	0·686	0·068	0·090
S_2 { κ =	273	275	274	29	34	24	250
S_4 { H =	0·007	0·005	0·006	0·007	......	......	0·002
S_4 { κ =	187	221	204	257	......	......	298
S_6 { H =							
S_6 { κ =							
S_8 { H =							
S_8 { κ =							
M_1 { H =	0·046	0·051	0·049	0·045	......	0·007	0·010
M_1 { κ =	115	98	106	230	......	26	319
M_2 { H =	1·718	1·712	1·715	2·001	1·996	0·116	0·190
M_2 { κ =	275	277	276	338	347	11	252
M_3 { H =	0·007	0·007	0·007	......	......	......	0·004
M_3 { κ =	17	21	19	......	......	......	9
M_4 { H =	0·028	0·027	0·028	0·015	0·024	......	0·011
M_4 { κ =	205	200	203	202	268	......	349
M_6 { H =	0·010	0·013	0·012	......	......	......	0·002
M_6 { κ =	88	80	84	......	......	......	152
M_8 { H =	......	......	......	......	......	......	0·001
M_8 { κ =	......	......	......	......	......	......	146
O { H =	0·694	0·698	0·696	0·443	0·488	0·479	0·059
O { κ =	77	78	78	164	162	315	302
K_1 { H =	1·097	1·095	1·096	0·899	0·901	0·525	0·116
K_1 { κ =	94	95	94	216	243	325	3
K_2 { H =	0·210	0·203	0·207	0·175	0·151	0·028	0·024
K_2 { κ =	260	267	263	29	54	288	254
P { H =	0·352	0·361	0·357	0·216	0·215	0·156	0·041
P { κ =	91	90	90	218	222	321	0
J { H =	0·068	0·100	0·084	......	......	0·035	0·008
J { κ =	96	103	99	......	......	297	15

Table II.

(a) *San Diego.* (b) *Port Leopold.* (c) *Beechey Island.* (d) *Cat Island.* (e) *Toulon.*

(a) Com. 0 h., Jan. 1. (b) Com. 0 h., Nov. 1, 1848, to July 31, 1849. (c) Com. 0 h., Nov. 2, 1858, to Feb. 28, 1859. (d) Com. 0 h., Jan. 1. (e) Com. 0 h., Jan. 1.

		(a)	(a)	(a)	(b)	(c)	(d)	(e)
Year		1860.	1861.	Mean.	1848–9.	1858–9.	1848.	1853.
Q	H =	0·129	0·160	0·145	......	......	0·091	0·006
	κ =	73	77	75	......	......	307	242
L	H =	0·033	0·005	0·019	0·044	0·080	0·012	0·007
	κ =	0	328	344	3	47	33	224
N	H =	0·415	0·440	0·428	0·420	0·429	0·026	0·046
	κ =	258	261	260	306	315	33	240
λ	H =	0·069	0·049	0·059	......	......	......	0·003
	κ =	179	268	224	......	......	......	10
ν	H =	0·134	0·070	0·102	......	......	......	0·008
	κ =	261	233	247	......	......	......	219
μ	H =	0·039	0·015	0·027	......	......	......	0·007
	κ =	244	235	240	......	......	......	219
R	H =	......	0·010	0·010				
	κ =	......	153	153				
T	H =	......	0·041	0·041				
	κ =	......	319	319				
MS	H =	0·006	0·012	0·009				
	κ =	188	191	189				
2SM	H =							
	κ =							
Mm	H =	......	......	......	......	......	0·094	0·061
	κ =	......	......	......	......	......	304	228
Mf	H =	......	......	......	......	......	0·069	0·045
	κ =	......	......	......	......	......	134	118
MSf	H =	......	......	......	......	......	0·095	0·018
	κ =	......	......	......	......	......	336	53
Sa	H =	......	......	......	......	......	0·274	0·157
	κ =	......	......	......	......	......	145	279
Ssa	H =	......	......	......	......	......	0·128	0·090
	κ =	......	......	......	......	......	35	144

Table II.

(*a*) *Brest.* (*b*) *Ramsgate.* (*c*) *West Hartlepool.*

(*a*) Com. 0 h., Jan. 1. (*b*) Com. 0 h., Jan. 1. (*c*) Com. 0 h., July 1.

N.B.—English ports referred to G.M.T.

	(*a*)	(*b*)	(*c*)	(*c*)	(*c*)	(*c*)
Year	1875.	1864.	1858–9.	1859–60.	1860–1.	Mean.
S_1 H =	0·015	0·037	0·019	0·054	0·025	0·033
S_1 κ =	52	313	132	157	169	152
S_2 H =	2·551	1·877	1·754	1·711	1·749	1·738
S_2 κ =	138	33	141	138	138	139
S_4 H =	......	0·032	0·025	0·021	0·019	0·022
S_4 κ =	......	4	190	174	172	179
S_6 H =	......	0·027				
S_6 κ =	......	27				
S_8 H =						
S_8 κ =						
M_1 H =	0·004	......	0·028	0·030	0·019	0·026
M_1 κ =	167	......	39	125	147	104
M_2 H =	6·766	6·144	5·176	5·148	5·166	5·163
M_2 κ =	100	341	99	99	97	98
M_3 H =	0·067	0·043	0·038	0·023	0·046	0·036
M_3 κ =	2	56	122	105	127	118
M_4 H =	0·169	0·548	0·080	0·106	0·099	0·095
M_4 κ =	85	243	103	117	107	109
M_6 H =	0·106	0·164	0·071	0·078	0·073	0·074
M_6 κ =	325	127	50	55	46	50
M_8 H =	0·008	0·054				
M_8 κ =	203	54				
O H =	0·211	0·342	0·433	0·425	0·444	0·434
O κ =	322	180	84	86	85	85
K_1 H =	0·208	0·223	0·390	0·365	0·385	0·380
K_1 κ =	66	18	247	247	248	248
K_2 H =	0·553	0·520	0·485	0·511	0·467	0·488
K_2 κ =	144	24	139	136	132	135
P H =	0·071	0·073	0·121	0·120	0·095	0·112
P κ =	59	353	232	232	232	232
J H =	......	......	0·031	0·026	0·027	0·028
J κ =	......	......	268	300	105	224

Table II.

(a) Brest. *(b) Ramsgate.* *(c) West Hartlepool.*

(*a*) Com. 0 h., Jan. 1. (*b*) Com. 0 h., Jan. 1. (*c*) Com. 0 h., July 1.

N.B.—English ports referred to G.M.T.

		(*a*)	(*b*)	(*c*)	(*c*)	(*c*)	(*c*)
Year		1875.	1864.	1858–9.	1859–60.	1860–1.	Mean.
Q	H =	......	......	0·140	0·143	0·160	0·148
	κ =	......	......	41	31	25	32
L	H =	0·192	0·447	0·169	0·179	0·253	0·200
	κ =	101	16	106	140	94	114
N	H =	1·375	1·084	0·951	0·973	1·040	0·988
	κ =	83	312	77	70	72	73
λ	H =	0·059	0·174	0·057	0·110	0·117	0·095
	κ =	59	351	148	85	115	116
ν	H =	0·293	0·344	0·115	0·325	0·369	0·270
	κ =	45	330	75	116	73	88
μ	H =	0·307	0·251	0·097	0·100	0·057	0·085
	κ =	92	87	9	346	24	6
R	H =	......	......	......	......	0·008	0·008
	κ =	......	......	......	......	158	158
T	H =	......	......	......	......	0·140	0·140
	κ =	......	......	......	......	200	200
MS	H =	......	0·324	0·047	0·040	0·046	0·044
	κ =	......	127	122	142	115	126
2SM	H =	......	0·141	0·034	0·034	0·009	0·026
	κ =	......	262	315	29	226	310
Mm	H =	0·038	0·029	0·085	0·148	0·147	0·127
	κ =	328	45	24	176	79	93
Mf	H =	0·069	0·044	0·037	0·040	0·060	0·046
	κ =	76	288	200	237	178	205
MSf	H =	0·290	0·094	0·135	0·134	0·143	0·137
	κ =	52	206	70	56	53	59
Sa	H =	0·261	0·127	0·217	0·366	0·213	0·265
	κ =	234	181	258	200	200	219
Ssa	H =	0·071	0·075	0·004	0·138	0·149	0·097
	κ =	93	288	275	106	287	223

Table II.

Portland Breakwater.

Commence 0 h., January 1.

N.B.—Referred to G.M.T.

Year	1851.	1857.	1866.	1870.	Mean.
S_1 { H =	0·074	0·031	0·026	0·015	0·037
κ =	84	98	91	83	89
S_2 { H =	1·076	1·076	1·090	1·055	1·074
κ =	243	247	245	241	244
S_4 { H =	0·012	0·010	0·016	0·010	0·012
κ =	193	185	168	196	186
S_6 { H =					
κ =					
S_8 { H =					
κ =					
M_1 { H =	0·011	0·004	0·030	0·013	0·015
κ =	317	184	278	32	292
M_2 { H =	2·109	2·104	1·911	2·067	2·048
κ =	193	197	195	192	194
M_3 { H =	0·029	0·045	0·045	0·026	0·036
κ =	172	195	188	166	180
M_4 { H =	0·440	0·535	0·439	0·456	0·468
κ =	29	42	31	29	32
M_6 { H =	0·211	0·217	0·195	0·203	0·207
κ =	67	79	68	65	70
M_8 { H =	0·013	0·017	0·009	0·009	0·012
κ =	54	46	40	57	49
O { H =	0·165	0·162	0·156	0·168	0·163
κ =	351	357	351	353	353
K_1 { H =	0·283	0·292	0·295	0·308	0·295
κ =	113	116	114	114	114
K_2 { H =	0·312	0·292	0·316	0·282	0·301
κ =	238	243	234	236	237
P { H =	0·096	0·118	0·108	0·108	0·108
κ =	111	108	105	108	108
J { H =					
κ =					

Table II.

Portland Breakwater.

Commence 0 h., January 1.

N.B.—Referred to G.M.T.

Year		1851.	1857.	1866.	1870.	Mean.
Q	H =					
	κ =					
L	H =	0·227	0·105	0·142	0·208	0·171
	κ =	144	98	109	95	111
N	H =	0·465	0·462	0·499	0·483	0·477
	κ =	184	186	186	184	185
λ	H =	0·103	0·058	0·080	0·089	0·083
	κ =	113	109	134	112	117
ν	H =	0·093	0·125	0·121	0·121	0·115
	κ =	196	119	109	135	140
μ	H =	0·377	0·401	0·350	0·367	0·374
	κ =	197	199	193	193	196
R	H =					
	κ =					
T	H =					
	κ =					
MS	H =	0·261	0·276	0·253	0·279	0·267
	κ =	86	94	90	91	90
2SM	H =	0·050	0·072	0·062	0·050	0·059
	κ =	351	6	348	346	353
Mm	H =					
	κ =					
Mf	H =					
	κ =					
MSf	H =					
	κ =					
Sa	H =					
	κ =					
Ssa	H =					
	κ =					

Table II.

Liverpool.

Commence 0 h., September 1.

N.B.—Referred to G.M.T.

Year		1857–8.	1858–9.	1859–60.	Mean.
S_1	H =	0·045	0·070	0·084	0·066
	κ =	70	60	57	62
S_2	H =	3·215	3·312	3·194	3·240
	κ =	12	11	10	11
S_4	H =	0·061	0·060	0·048	0·056
	κ =	322	330	295	316
S_6	H =				
	κ =				
S_8	H =				
	κ =				
M_1	H =	0·015	0·042	0·004	0·020
	κ =	303	314	159	258
M_2	H =	10·033	10·136	10·130	10·100
	κ =	327	327	326	326
M_3	H =	0·111	0·103	0·159	0·124
	κ =	331	317	324	324
M_4	H =	0·737	0·700	0·668	0·702
	κ =	221	220	225	222
M_6	H =	0·202	0·208	0·224	0·211
	κ =	344	352	348	348
M_8	H =	0·067	0·092	0·073	0·077
	κ =	264	283	266	271
O	H =	0·374	0·356	0·400	0·377
	κ =	45	42	42	43
K_1	H =	0·354	0·362	0·357	0·358
	κ =	195	197	189	194
K_2	H =	0·904	1·001	0·912	0·939
	κ =	9	9	3	7
P	H =	0·125	0·134	0·131	0·130
	κ =	192	196	189	192
J	H =				
	κ =				

Table II.

Liverpool.

Commence 0 h., September 1.

N.B.—Referred to G.M.T.

Year		1857–8.	1858–9.	1859–60.	Mean.
Q	H =				
	κ =				
L	H =	0·408	0·681	0·530	0·540
	κ =	330	4	342	345
N	H =	1·930	1·819	2·019	1·923
	κ =	304	310	306	306
λ	H =	0·424	0·233	0·120	0·259
	κ =	322	316	13	337
ν	H =	0·769	0·651	0·291	0·570
	κ =	308	285	263	285
μ	H =	0·308	0·241	0·323	0·291
	κ =	33	44	36	38
R	H =	....	0·101	0·082	0·092
	κ =	....	46	46	46
T	H =	....	0·349	0·121	0·235
	κ =	....	348	317	333
MS	H =	0·454	0·361	0·397	0·404
	κ =	271	267	272	270
2SM	H =	0·140	0·165	0·151	0·152
	κ =	206	216	228	216
Mm	H =	0·053	0·223	0·166	0·147
	κ =	289	32	173	165
Mf	H =	0·064	0·027	0·018	0·036
	κ =	175	159	89	141
MSf	H =	0·071	0·021	0·081	0·058
	κ =	111	324	302	246
Sa	H =	0·359	0·284	0·353	0·332
	κ =	210	259	213	227
Ssa	H =	0·090	0·104	0·190	0·128
	κ =	144	270	112	175

Table II.

Liverpool.

Commence 0 h., January 23.

N.B.—Referred to G.M.T.

Year	1866–7.	1867–8.	1868–9.	1869–70.	Mean.
S_1 H =	0·047	0·035	0·040	0·028	0·038
S_1 κ =	39	66	101	124	83
S_2 H =	3·130	3·099	3·122	3·052	3·101
S_2 κ =	12	11	12	14	12
S_4 H =	0·048	0·068	0·064	0·051	0·058
S_4 κ =	314	327	298	313	313
S_6 H =					
S_6 κ =					
S_3 H =					
S_3 κ =					
M_1 H =	0·035	0·023	0·049	0·048	0·039
M_1 κ =	304	261	19	39	336
M_2 H =	9·901	9·906	9·807	9·911	9·881
M_2 κ =	326	326	327	328	327
M_3 H =	0·081	0·097	0·111	0·098	0·097
M_3 κ =	336	327	323	311	324
M_4 H =	0·711	0·673	0·659	0·688	0·683
M_4 κ =	225	222	221	225	223
M_6 H =	0·184	0·174	0·172	0·205	0·184
M_6 κ =	344	347	350	358	350
M H =	0·058	0·058	0·059	0·070	0·061
M κ =	283	279	291	287	285
O H =	0·379	0·331	0·398	0·357	0·366
O κ =	41	41	39	36	40
K_1 H =	0·372	0·332	0·344	0·362	0·353
K_1 κ =	192	197	194	196	195
K_2 H =	0·846	1·021	0·935	0·933	0·934
K_2 κ =	10	9	3	7	7
P H =	0·141	0·136	0·133	0·094	0·126
P κ =	178	199	174	167	180
J H =					
J κ =					

Table II.

Liverpool.

Commence 0 h., January 23.

N.B.—Referred to G.M.T.

Year	1866–7.	1867–8.	1868–9.	1869–70.	Mean.
Q { H =					
Q { κ =					
L { H =	0·550	0·491	0·476	0·565	0·521
L { κ =	296	331	347	337	328
N { H =	2·083	1·845	1·774	1·848	1·888
N { κ =	302	308	306	303	305
λ { H =	0·228	0·209	0·192	0·187	0·204
λ { κ =	356	0	317	310	336
ν { H =	0·692	0·487	0·138	0·675	0·498
ν { κ =	279	267	310	331	297
μ { H =	0·233	0·212	0·242	0·220	0·228
μ { κ =	33	31	62	36	41
R { H =					
R { κ =					
T { H =					
T { κ =					
MS { H =	0·447	0·400	0·395	0·387	0·407
MS { κ =	270	271	268	271	270
2SM { H =	0·134	0·112	0·136	0·118	0·125
2SM { κ =	222	225	225	235	227
Mm. { H =	0·064	....	....	....	0·064
Mm. { κ =	260	....	....	....	260
Mf { H =	0·057	....	....	....	0·057
Mf { κ =	344	....	....	....	344
MSf { H =	0·051	....	....	....	0·051
MSf { κ =	68	....	....	....	68
Sa { H =	0·452	....	....	....	0·452
Sa { κ =	272	....	....	....	272
Ssa { H =	0·185	....	....	....	0·185
Ssa { κ =	229	....	....	....	229

Table II.

Helbre Island.

Commence 0 h., January 1.

N.B.—Referred to G.M.T.

Year		1858.	1859.	1860.	1861.	1862.	1863.
S_1	H =						
	κ =						
S_2	H =	3·138	3·177	3·163	3·171	3·119	3·120
	κ =	2	3	2	2	3	3
S_4	H =	0·033	0·033	0·026	0·030	0·026	0·025
	κ =	322	329	298	332	317	300
S_6	H =						
	κ =						
S_8	H =						
	κ =						
M_1	H =	0·008	0·043	0·036	0·023	0·080	0·013
	κ =	289	8	108	60	125	267
M_2	H =	9·768	9·763	9·929	9·828	9·740	9·709
	κ =	319	320	319	318	320	320
M_3	H =	0·104	0·091	0·106	0·146	0·079	0·117
	κ =	304	288	307	278	283	279
M_4	H =	0·446	0·441	0·491	0·479	0·409	0·500
	κ =	216	219	214	210	218	213
M_6	H =	0·065	0·063	0·081	0·066	0·065	0·066
	κ =	37	51	26	21	28	36
M_8	H =	0·013	0·007	0·012	0·013	0·011	0·013
	κ =	350	51	339	6	309	13
O	H =	0·367	0·368	0·361	0·404	0·379	0·377
	κ =	41	42	42	42	44	40
K_1	H =	0·387	0·376	0·376	0·404	0·387	0·388
	κ =	192	187	186	188	188	189
K_2	H =	0·922	0·883	0·919	0·916	0·928	0·989
	κ =	358	355	354	1	354	5
P	H =	0·172	0·147	0·131	0·131	0·162	0·138
	κ =	184	180	190	99	176	194
J	H =	0·022	0·018				
	κ =	251	159				

Table II.

Helbre Island.

Commence 0 h., January 1.

N.B.—Referred to G.M.T.

Year	1858.	1859.	1860.	1861.	1862.	1863.
Q { H =	0·108	0·099				
Q { κ =	359	334				
L { H =	0·370	0·561	0·477	0·424	0·215	0·315
L { κ =	334	354	336	324	256	353
N { H =	1·855	1·896	1·794	1·883	1·847	1·843
N { κ =	296	292	291	295	297	296
λ { H =	0·144	0·204	0·058	0·202	0·255	0·182
λ { κ =	327	293	14	353	357	323
ν { H =	0·189	0·321	0·221	0·626	0·371	0·611
ν { κ =	268	274	336	277	210	276
μ { H =	0·033	0·176	0·076	0·145	0·026	0·057
μ { κ =	80	44	30	32	345	73
R { H =	......	0·022	......	0·102		
R { κ =	......	18	......	291		
T { H =	......	0·222	......	0·406		
T { κ =	......	311	......	6		
MS { H =	0·275	0·206	0·261	0·310	0·257	0·270
MS { κ =	276	265	272	267	277	266
2SM { H =	0·132	0·122	0·123	0·126	0·126	0·123
2SM { κ =	217	230	237	208	216	234
Mm { H =						
Mm { κ =						
Mf { H =						
Mf { κ =						
MSf { H =						
MSf { κ =						
Sa { H =						
Sa { κ =						
Ssa { H =						
Ssa { κ =						

Table II.

Helbre Island.

Commence 0 h., January 1.

N.B.—Referred to G.M.T

Year	1864.	1865.	1866.	1867.	Mean.
S_1 { H =					
κ =					
S_2 { H =	3·089	3·093	3·106	3·108	3·128
κ =	3	3	1	2	3
S_4 { H =	0·035	0·029	0·030	0·034	0·030
κ =	309	303	304	302	312
S_6 { H =					
κ =					
S_8 { H =					
κ =					
M_1 { H =	0·002	0·034	0·044	0·046	0·033
κ =	166	256	336	284	262
M_2 { H =	9·728	9·762	9·708	9·645	9·758
κ =	321	320	319	319	319
M_3 { H =	0·104	0·077	0·107	0·110	0·104
κ =	305	285	310	293	293
M_4 { H =	0·515	0·510	0·503	0·494	0·479
κ =	213	211	209	211	213
M_6 { H =	0·078	0·069	0·079	0·072	0·070
κ =	44	42	32	28	34
M_8 { H =	0·011	0·009	0·009	0·005	0·010
κ =	18	348	307	338	352
O { H =	0·341	0·386	0·363	0·357	0·370
κ =	37	41	40	39	41
K_1 { H =	0·388	0·416	0·419	0·370	0·391
κ =	189	189	185	187	188
K_2 { H =	0·738	0·919	0·918	0·770	0·890
κ =	0	4	351	357	358
P { H =	0·134	0·153	0·160	0·134	0·146
κ =	176	179	179	183	174
J { H =	......	......	0·018	0·044	0·026
κ =	......	......	342	98	122

Table II.

Helbre Island.

Commence 0 h., January 1.

N.B.—Referred to G.M.T.

Year ……		1864.	1865.	1866.	1867.	Mean.
Q	H =	……	……	0·121	0·122	0·113
	κ =	……	……	339	350	345
L	H =	0·506	0·554	0·390	0·466	0·428
	κ =	356	342	317	341	331
N	H =	1·923	1·852	1·824	1·849	1·857
	κ =	296	295	298	297	295
λ	H =	0·275	0·263	0·254	0·106	0·194
	κ =	334	343	343	290	334
ν	H =	0·666	0·683	0·522	0·173	0·438
	κ =	289	295	263	291	278
μ	H =	0·061	0·095	0·012	0·151	0·083
	κ =	11	43	331	66	34
R	H =	……	0·050	……	0·026	0·050
	κ =	……	344	……	63	359
T	H =	……	0·230	……	0·158	0·254
	κ =	……	356	……	277	327
MS	H =	0·314	0·322	0·290	0·292	0·280
	κ =	266	264	261	260	267
2SM	H =	0·094	0·124	0·104	0·112	0·119
	κ =	212	210	221	230	221
Mm	H =					
	κ =					
Mf	H =					
	κ =					
MSf	H =					
	κ =					
Sa	H =					
	κ =					
Ssa	H =					
	κ =					

Table III.

1, *Aden*. 2, *Karachi*. 3, *Okha*. 4, *Kathiwadar*. 5, *Bombay*. 6, *Karwar*. 7, *Beypore*.

	1	2	3	4	5	6	7
Years.	1879–83.	1868–83.	1874–5.	1881–2.	1878–82.	1878–83.	1878–83.
No. of years observed ...	4	15	1	1	5	5	5
S_1 H =	0·090	0·082	0·074	0·134	0·078	0·057	0·061
S_1 κ =	162	158	150	201	182	159	174
S_2 H =	0·697	0·948	1·222	1·207	1·622	0·624	0·330
S_2 κ =	248	322	14	81	3	335	18
S_4 H =	0·006	0·010	0·013	0·029	0·012	0·010	0·005
S_4 κ =	271	14	117	273	256	100	137
S_6 H =	0·004	0·007	0·003	0·013	0·003	0·005	0·005
S_6 κ =	201	295	21	42	171	52	247
S_8 H =	0·001	0·001	0·001	0·002	0·001	0·002	0·001
S_8 κ =	259	204	220	264	115	304	339
M_1 H =	0·047	0·044	0·051	0·057	0·051	0·033	0·029
M_1 κ =	21	30	43	35	49	41	73
M_2 H =	1·568	2·504	3·820	2·970	4·034	1·742	0·931
M_2 κ =	229	294	347	55	330	302	329
M_3 H =	0·018	0·039	0·030	0·020	0·065	0·014	0·010
M_3 κ =	209	330	21	152	23	273	197
M_4 H =	0·007	0·024	0·136	0·220	0·124	0·055	0·020
M_4 κ =	314	14	107	178	322	17	41
M_6 H =	0·005	0·049	0·007	0·139	0·011	0·011	0·007
M_6 κ =	341	210	270	137	111	284	138
M_8 H =	0·003	0·005	0·011	0·002	0·004	0·002	0·008
M_8 κ =	43	267	96	199	351	109	146
O H =	0·653	0·647	0·693	0·720	0·650	0·497	0·340
O κ =	38	47	57	66	48	49	57
K_1 H =	1·299	1·281	1·414	1·611	1·393	1·004	0·704
K_1 κ =	36	46	53	66	45	45	52
K_2 H =	0·201	0·278	0·328	0·324	0·410	0·174	0·080
K_2 κ =	244	320	17	79	352	330	11
P H =	0·388	0·380	0·384	0·436	0·402	0·277	0·191
P κ =	33	46	50	71	42	42	53
J H =	0·103	0·079	0·107	0·175	0·089	0·068	0·044
J κ =	52	70	81	107	72	57	63

Table III.

1, *Aden.* 2, *Karachi.* 3, *Okha.* 4, *Kathiwadar.* 5, *Bombay.* 6, *Karwar.* 7, *Beypore.*

		1	2	3	4	5	6	7
Years.		1879–83.	1868–83.	1874–5.	1881–2.	1878–82.	1878–83.	1878–83.
No. of years observed ...		4	15	1	1	5	5	5
Q	H =	0·151	0·129	0·137	0·152	0·131	0·114	0·081
	κ =	42	52	59	68	52	59	66
L	H =	0·046	0·081	0·221	0·079	0·108	0·056	0·027
	κ =	230	299	23	261	316	317	348
N	H =	0·427	0·600	0·781	0·755	1·003	0·410	0·197
	κ =	225	277	322	34	314	282	305
λ	H =	0·026	0·042	0·073	0·043	0·032	0·020	0·011
	κ =	197	282	23	107	235	273	313
ν	H =	0·099	0·142	0·164	0·131	0·199	0·088	0·055
	κ =	226	277	8	15	315	294	311
μ	H =	0·075	0·061	0·203	0·286	0·206	0·044	0·019
	κ =	196	263	182	343	308	263	258
R	H =	0·005	0·030	......	......	0·042	0·008	0·023
	κ =	30	276	......	......	283	145	132
T	H =	0·050	0·068	......	......	0·171	0·061	0·040
	κ =	240	332	......	......	24	155	19
MS	H =	0·012	0·027	0·064	0·159	0·129	0·026	0·009
	κ =	159	307	111	215	24	67	77
2SM	H =	0·023	0·021	0·044	0·029	0·036	0·007	0·005
	κ =	109	123	292	154	106	315	296
Mm	H =	0·042	0·060	0·066	0·052	0·056	0·065	0·091
	κ =	354	95	311	8	26	27	32
Mf	H =	0·045	0·033	0·050	0·027	0·051	0·042	0·071
	κ =	31	316	44	103	346	5	23
MSf	H =	0·014	0·036	0·141	0·040	0·031	0·022	0·038
	κ =	341	266	250	153	228	164	216
Sa	H =	0·390	0·138	0·162	0·236	0·186	0·352	0·309
	κ =	357	79	3	133	358	310	313
Ssa	H =	0·095	0·135	0·121	0·109	0·122	0·068	0·177
	κ =	126	142	145	156	228	228	205

Table III.

8, *Paumben*. 9, *Negapatam*. 10. *Madras*. 11. *Vizagapatam*. 12. *False Point*. 13. *Dublat*. 14. *Diamond Harbour*.

		8	9	10	11	12	13	14
Years.		1878–82.	1881–3.	1880–3.	1879–83.	1881–3.	1881–3.	1881–3.
No. of years observed ...		4	2	3	4	2	2	2
S_1	H =	0·036	0·046	0·025	0·052	0·015	0·047	0·085
	κ =	148	108	92	68	6	110	152
S_2	H =	0·372	0·274	0·441	0·656	1·018	2·108	2·252
	κ =	92	284	276	285	303	327	26
S_4	H =	0·003	0·005	0·002	0·005	0·008	0·018	0·120
	κ =	261	151	161	47	329	211	326
S_6	H =	0·004	0·000	0·001	0·001	0·003	0·004	0·013
	κ =	197	150	112	170	169	99	251
S_8	H =	0·003	0·001	0·001	0·002	0·003	0·006	0·003
	κ =	224	227	162	79	240	113	353
M_1	H =	0·011	0·005	0·008	0·012	0·009	0·008	0·020
	κ =	35	111	7	295	30	41	95
M_2	H =	0·585	0·720	1·049	1·473	2·250	4·610	5·177
	κ =	47	252	248	253	270	290	344
M_3	H =	0·016	0·003	0·004	0·006	0·014	0·046	0·035
	κ =	170	103	62	329	30	133	223
M_4	H =	0·016	0·021	0·003	0·015	0·038	0·095	0·745
	κ =	194	77	146	331	230	144	246
M_6	H =	0·011	0·012	0·010	0·005	0·010	0·014	0·152
	κ =	42	128	154	72	63	255	106
M_8	H =	0·005	0·005	0·002	0·003	0·003	0·012	0·062
	κ =	314	303	46	209	246	294	345
O	H =	0·115	0·091	0·097	0·141	0·177	0·189	0·234
	κ =	45	322	325	331	335	334	345
K_1	H =	0·294	0·225	0·293	0·359	0·408	0·493	0·496
	κ =	46	345	340	342	345	352	14
K_2	H =	0·113	0·077	0·112	0·203	0·255	0·596	0·656
	κ =	90	286	280	277	297	318	23
P	H =	0·110	0·084	0·097	0·096	0·145	0·155	0·175
	κ =	46	346	344	339	344	343	9
J	H =	0·014	0·011	0·021	0·025	0·026	0·024	0·031
	κ =	48	328	318	337	312	324	320

Table III.

8, *Paumben.* 9, *Negapatam.* 10, *Madras.* 11, *Vizagapatam.* 12, *False Point.* 13, *Dublat.* 14, *Diamond Harbour.*

	8	9	10	11	12	13	14
Years.	1878–82.	1881–3.	1880–3.	1879–83.	1881–3.	1881–3.	1881–3.
No. of years observed ...	4	2	3	4	2	2	2
Q { H =	0·021	0·007	0·005	0·009	0·011	0·009	0·030
Q { κ =	89	181	111	325	324	333	9
L { H =	0·023	0·027	0·036	0·052	0·059	0·167	0·261
L { κ =	58	279	307	254	254	291	351
N { H =	0·082	0·158	0·240	0·314	0·476	0·947	0·951
N { κ =	31	244	242	246	267	286	339
λ { H =	0·016	0·015	0·029	0·022	0·063	0·219	0·115
λ { κ =	64	231	299	264	180	316	337
ν { H =	0·027	0·048	0·044	0·075	0·142	0·232	0·303
ν { κ =	30	217	271	199	244	251	289
μ { H =	0·009	0·021	0·041	0·027	0·075	0·165	0·303
μ { κ =	105	122	178	259	273	15	85
R { H =	0·016	0·031	0·016	0·027	0·034	0·219	0·216
R { κ =	114	349	103	188	217	289	10
T { H =	0·025	0·050	0·056	0·051	0·017	0·137	0·078
T { κ =	92	255	257	263	149	299	55
MS { H =	0·018	0·018	0·003	0·012	0·041	0·077	0·695
MS { κ =	292	96	170	5	274	155	285
2SM { H =	0·010	0·007	0·022	0·012	0·017	0·072	0·074
2SM { κ =	333	188	209	225	187	211	271
Mm { H =	0·048	0·057	0·047	0·055	0·063	0·040	0·102
Mm { κ =	27	328	80	54	55	77	1
Mf { H =	0·043	0·039	0·045	0·042	0·067	0·048	0·150
Mf { κ =	355	7	6	2	35	66	39
MSf { H =	0·016	0·091	0·019	0·046	0·050	0·063	0·451
MSf { κ =	141	7	58	23	356	356	33
Sa { H =	0·149	0·533	0·385	0·714	0·793	0·900	1·100
Sa { κ =	302	232	212	182	166	150	143
Ssa { H =	0·157	0·358	0·305	0·332	0·287	0·208	0·066
Ssa { κ =	108	130	128	114	146	136	71

Table III.

15, *Kidderpore*. 16, *Elephant Point*. 17, *Rangoon*. 18, *Amherst*. 19, *Moulmein*. 20, *Port Blair*. 21, *Fort Point*.

		15	16	17	18	19	20	21
Years.		1881–3.	1880–1.	1880–3.	1880–3.	1880–3.	1880–3.	1858–61.
No. of years observed ...		2	1	3	3	3	3	3
S_1	H =	0·091	0·113	0·113	0·222	0·096	0·021	0·015
	κ =	193	79	133	141	149	38	212
S_2	H =	1·468	2·337	2·012	2·769	1·362	0·968	0·390
	κ =	101	143	170	105	148	315	336
S_4	H =	0·075	0·037	0·081	0·106	0·067	0·003	
	κ =	119	162	259	122	229	84	
S_6	H =	0·005	0·021	0·010	0·012	0·005	0·002	
	κ =	299	94	48	187	183	131	
S_8	H =	0·008	0·008	0·005	0·008	0·002	0·002	
	κ =	311	60	120	276	211	80	
M_1	H =	0·013	0·019	0·033	0·032	0·018	0·010	0·038
	κ =	157	88	183	255	138	291	170
M_2	H =	3·627	5·870	5·545	6·233	3·779	2·022	1·689
	κ =	58	103	131	69	113	278	332
M_3	H =	0·015	0·025	0·021	0·019	0·025	0·007	
	κ =	331	146	178	261	209	16	
M_4	H =	0·727	0·079	0·410	0·350	0·901	0·008	0·071
	κ =	37	46	169	51	171	151	24
M_6	H =	0·159	0·205	0·233	0·118	0·102	0·002	
	κ =	319	349	87	252	200	317	
M_8	H =	0·078	0·031	0·081	0·014	0·038	0·002	
	κ =	270	322	97	249	133	70	
O	H =	0·220	0·349	0·294	0·317	0·253	0·160	0·780
	κ =	21	356	28	339	48	302	87
K_1	H =	0·389	0·807	0·670	0·699	0·438	0·397	1·219
	κ =	56	18	35	5	40	327	107
K_2	H =	0·435	0·401	0·570	1·104	0·336	0·282	0·135
	κ =	96	91	169	90	155	311	330
P	H =	0·144	0·199	0·149	0·177	0·134	0·134	0·373
	κ =	47	33	55	337	60	326	105
J	H =	0·014	0·110	0·030	0·074	0·022	0·027	0·053
	κ =	327	61	33	34	107	325	121

Table III.

15, *Kidderpore.* 16, *Elephant Point.* 17, *Rangoon.* 18, *Amherst.* 19, *Moulmein.* 20, *Port Blair,* 21, *Fort Point.*

	15	16	17	18	19	20	21
Years.	1881–3.	1880–1.	1880–3.	1880–3.	1880–3.	1880–3.	1858–61.
No. of years observed ...	2	1	3	3	3	3	3
Q { H =	0·039	0·042	0·027	0·054	0·045	0·024	0·121
Q { κ =	9	336	31	322	53	237	74
L { H =	0·187	0·346	0·407	0·292	0·279	0·068	0·059
L { κ =	74	109	157	112	139	272	338
N { H =	0·638	1·543	0·990	1·322	0·679	0·399	0·374
N { κ =	47	80	117	54	102	274	305
λ { H =	0·101	0·659	0·257	0·300	0·176	0·043	0·026
λ { κ =	107	145	170	119	165	280	345
ν { H =	0·238	0·681	0·317	0·425	0·233	0·121	0·064
ν { κ =	353	209	100	177	84	254	305
μ { H =	0·242	0·356	0·514	0·303	0·313	0·086	0·029
μ { κ =	182	279	290	301	270	292	227
R { H =	0·167	......	0·117	0·451	0·097	0·020	0·008
R { κ =	77	......	66	252	70	326	63
T { H =	0·147	......	0·290	0·841	0·200	0·099	0·014
T { κ =	107	......	128	144	110	313	198
MS { H =	0·645	0·135	0·386	0·347	0·712	0·009	0·031
MS { κ =	81	67	210	82	211	215	21
2SM { H =	0·085	0·042	0·160	0·151	0·124	0·023	
2SM { κ =	2	84	54	10	38	154	
Mm { H =	0·244	0·145	0·236	0·095	0·360	0·014	
Mm { κ =	351	6	23	48	14	20	
Mf { H =	0·297	0·098	0·208	0·097	0·334	0·057	
Mf { κ =	38	310	34	350	41	9	
MSf { H =	0·875	0·059	0·554	0·055	1·110	0·015	
MSf { κ =	39	273	49	71	46	61	
Sa { H =	2·740	0·930	1·486	0·726	2·434	0·204	
Sa { κ =	157	146	150	140	149	150	
Ssa { H =	0·822	0·261	0·126	0·156	0·603	0·117	
Ssa { κ =	269	198	328	235	287	177	

Table III.

22, *San Diego*. 23, *Port Leopold*. 24, *Beechey Island*. 25, *Cat Island, Gulf of Mexico*. 26, *Toulon*. 27, *Brest*. 28, *Ramsgate* (*referred to G.M.T.*).

		22	23	24	25	26	27	28
Years.		1860–1.	1848–9.	1858–9.	1848.	1853.	1875.	1864.
No. of years observed ...		2	1	1	1	1	1	1
S_1	H =	0·028	0·031	......	0·044	0·010	0·015	0·037
	κ =	238	27	......	10	186	52	313
S_2	H =	0·695	0·643	0·686	0·068	0·090	2·551	1·877
	κ =	274	29	34	24	250	138	33
S_4	H =	0·006	0·007	......	......	0·002	......	0·032
	κ =	204	257	......	......	298	......	4
S_6	H =	......	......	......	......	......	......	0·027
	κ =	......	......	......	......	......	......	27
S_8	H =							
	κ =							
M_1	H =	0·049	0·045	......	0·007	0·010*	0·004	
	κ =	106	230	......	26	319	167	
M_2	H =	1·715	2·001	1·996	0·116	0·190	6·766	6·144
	κ =	276	338	347	11	252	100	341
M_3	H =	0·007	......	......	......	0·004	0·067	0·043
	κ =	19	......	......	......	9	2	56
M_4	H =	0·028	0·015	0·024	......	0·011	0·169	0·548
	κ =	203	202	268	......	349	85	243
M_6	H =	0·012	......	......	......	0·002	0·106	0·164
	κ =	84	......	......	......	152	325	127
M_8	H =	......	......	......	......	0·001	0·008	0·054
	κ =	......	......	......	......	146	203	54
O	H =	0·696	0·443	0·488	0·479	0·059	0·211	0·342
	κ =	78	164	162	315	302	322	180
K_1	H =	1·096	0·899	0·901	0·525	0·116	0·208	0·223
	κ =	94	216	243	325	3	66	18
K_2	H =	0·207	0·175	0·151	0·028	0·024	0·553	0·520
	κ =	263	29	54	288	254	144	24
P	H =	0·357	0·216	0·215	0·156	0·041	0·071	0·073
	κ =	90	218	222	321	0	59	353
J	H =	0·084	......	......	0·035	0·008		
	κ =	99	......	......	297	15		

Table III.

22, *San Diego.* 23, *Port Leopold.* 24, *Beechey Island.* 25, *Cat Island, Gulf of Mexico.* 26, *Toulon.* 27, *Brest.* 28, *Ramsgate* (*referred to G.M.T.*).

	22	23	24	25	26	27	28
Years.	1860–1.	1848–9.	1858–9.	1848.	1853.	1875.	1864.
No. of years observed ...	2	1	1	1	1	1	1
Q { H =	0·145	......	......	0·091	0·006		
Q { κ =	75	......	......	307	242		
L { H =	0·019	0·044	0·080	0·012	0·007	0·192	0·447
L { κ =	344	3	47	33	224	101	16
N { H =	0·428	0·420	0·429	0·026	0·046	1·375	1·084
N { κ =	260	306	315	33	240	83	312
λ { H =	0·059	......	......	......	0·003	0·059	0·174
λ { κ =	224	......	......	......	10	59	351
ν { H =	0·102	......	......	......	0·008	0·293	0·344
ν { κ =	247	......	......	......	219	45	330
μ { H =	0·027	......	......	......	0·007	0·307	0·251
μ { κ =	240	......	......	......	219	92	87
R { H =	0·010						
R { κ =	153						
T { H =	0·041						
T { κ =	319						
MS { H =	0·009	......	......	......	......	......	0·324
MS { κ =	189	......	......	......	......	......	127
2SM { H =	......	......	......	......	......	......	0·141
2SM { κ =	......	......	......	......	......	......	262
Mm { H =	......	......	......	0·094	0·061	0·038	0·029
Mm { κ =	......	......	......	304	228	328	45
Mf { H =	......	......	......	0·069	0·045	0·069	0·044
Mf { κ =	......	......	......	134	118	76	288
MSf { H =	......	......	......	0·095	0·018	0·290	0·094
MSf { κ =	......	......	......	336	53	52	206
Sa { H =	......	......	......	0·274	0·157	0·261	0·127
Sa { κ =	......	......	......	145	279	234	181
Ssa { H =	......	......	......	0·128	0·090	0·071	0·075
Ssa { κ =	......	......	......	35	144	93	288

Table III.

29, *West Hartlepool.* 30, *Portland Breakwater.* 31, *Liverpool.* 32, *Liverpool.* 33, *Helbre Island.* 34, *Freemantle, West Australia.* 35, *Mauritius, Port Louis.*

N.B.—English ports referred to G.M.T.

		29	30	31	32	33	34	35
Years.		1858–61.	1851, 57, 66, & 70.	1857–60.	1866–70.	1858–67.	1873–4.	1838–9.
No. of years observed ...		3	4	3	4	10	1	1
S_1	H =	0·033	0·037	0·066	0·038	......	0·039	0·013
	κ =	152	89	62	83	......	60	32
S_2	H =	1·738	1·074	3·240	3·101	3·128	0·145	0·331
	κ =	139	244	11	12	3	292	26
S_4	H =	0·022	0·012	0·056	0·058	0·030	0·004	0·003
	κ =	179	186	316	313	312	72	116
S_6	H =	......	......	......	......	......	......	0·002
	κ =	......	......	......	......	......	......	235
S_8	H =	......	......	......	......	......	......	0·001
	κ =	......	......	......	......	......	......	114
M_1	H =	0·026	0·015	0·020	0·039	0·033	0·025	0·004
	κ =	104	292	258	336	262	261	100
M_2	H =	5·163	2·048	10·100	9·881	9·758	0·159	0·433
	κ =	98	194	326	327	319	286	23
M_3	H =	0·036	0·036	0·124	0·097	0·104	0·008	0·016
	κ =	118	180	324	324	293	217	167
M_4	H =	0·095	0·468	0·702	0·683	0·479	0·010	0·004
	κ =	109	32	222	223	213	260	296
M_6	H =	0·074	0·207	0·211	0·184	0·070	0·007	0·005
	κ =	50	70	348	350	34	277	94
M_8	H =	......	0·012	0·077	0·061	0·010	0·005	0·001
	κ =	......	49	271	285	352	259	168
O	H =	0·434	0·163	0·377	0·366	0·370	0·372	0·140
	κ =	85	353	43	40	41	·291	98
K_1	H =	0·380	0·295	0·358	0·353	0·391	0·638	0·244
	κ =	248	114	194	195	188	300	121
K_2	H =	0·488	0·301	0·939	0·934	0·890	0·057	0·138
	κ =	135	237	7	7	358	288	23
P	H =	0·112	0·108	0·130	0·126	0·146	0·156	0·056
	κ =	232	108	192	180	174	297	132
J	H =	0·028	......	......	......	0·026	0·029	0·009
	κ =	224	......	......	......	122	310	118

Table III.

29, *West Hartlepool.* 30, *Portland Breakwater.* 31, *Liverpool.* 32, *Liverpool.* 33, *Helbre Island.* 34, *Freemantle, West Australia.* 35, *Mauritius, Port Louis.*

N.B.—English ports referred to G.M.T.

	29	30	31	32	33	34	35
Years.	1858–61.	1851, 57, 66, & 70.	1857–60.	1866–70.	1858–67.	1873–4.	1838–9.
No. of years observed ...	3	4	3	4	10	1	1
Q { H =	0·148	......	......	......	0·113	0·099	0·024
Q { κ =	32	......	......	......	345	290	78
L { H =	0·200	0·171	0·540	0·521	0·428	0·021	0·033
L { κ =	114	111	345	328	331	244	4
N { H =	0·988	0·477	1·923	1·888	1·857	0·041	0·137
N { κ =	73	185	306	305	295	340	32
λ { H =	0·095	0·083	0·259	0·204	0·194	0·006	0·018
λ { κ =	116	117	337	336	334	356	298
ν { H =	0·270	0·115	0·570	0·498	0·438	0·012	0·008
ν { κ =	88	140	285	297	278	232	257
μ { H =	0·085	0·374	0·291	0·228	0·083	0·016	0·019
μ { κ =	6	196	38	41	34	324	317
R { H =	0·008	......	0·092	......	0·050		
R { κ =	158	......	46	......	359		
T { H =	0·140	......	0·235	......	0·254		
T { κ =	200	......	333	......	327		
MS { H =	0·044	0·267	0·404	0·407	0·280		
MS { κ =	126	90	270	270	267		
2SM { H =	0·026	0·059	0·152	0·125	0·119		
2SM { κ =	310	353	216	227	221		
Mm { H =	0·127	......	0·147	0·064	......	0·079	0·047
Mm { κ =	93	......	165	260	......	147	297
Mf { H =	0·046	......	0·036	0·057	......	0·082	0·036
Mf { κ =	205	......	141	344	......	25	350
MSf { H =	0·137	......	0·058	0·051	......	0·032	0·015
MSf { κ =	59	......	246	68	......	178	91
Sa { H =	0·265	......	0·332	0·452	......	0·537	0·211
Sa { κ =	219	......	227	272	......	27	346
Ssa { H =	0·097	......	0·128	0·185	......	0·175	0·118
Ssa { κ =	223	......	175	229	......	126	118

Table III.

36, *Falkland Islands, Port Louis.* 37, *Malta.* 38, *Marseilles.* 39, *Toulon.*

		36	37	38	39
Years.		1842–3.	1871–2.	1850–1.	Mean of 1847, 48, 53.
No. of years observed ...		1	1	1	
S_1	H =	0·289	0·009	0·019	0·011
	κ =	25	162	48	20
S_2	H =	0·492	0·120	0·078	0·091
	κ =	195	100	247	250
S_4	H =	0·007	0·001	0·003	0·002
	κ =	64	37	277	288
S_6	H =				
	κ =				
S_8	H =				
	κ =				
M_1	H =	0·024	0·005	0·003	0·005
	κ =	79	69	124	168
M_2	H =	1·544	0·197	0·220	0·195
	κ =	157	93	228	246
M_3	H =	0·018	0·002	0·005	0·004
	κ =	83	204	185	174
M_4	H =	0·068	0·003	0·019	0·014
	κ =	357	350	0	352
M_6	H =	0·012	0·001	....	0·001
	κ =	76	26	....	145
M_8	H =	0·010	0·003	....	0 002
	κ =	193	127	....	60
O	H =	0·451	0·024	0·069	0·060
	κ =	4	83	106	120
K_1	H =	0·358	0·035	0·104	0·105
	κ =	37	43	181	186
K_2	H =	0·170	0·033	0·016	0·019
	κ =	206	110	254	254
P	H =	0·141	0·011	0·040	0·041
	κ =	87	58	182	178
J	H =	....	0·001	0·008	0·005
	κ =	....	59	198	176

Table III.

36, *Falkland Islands, Port Louis.* 37, *Malta.* 38, *Marseilles.* 39, *Toulon.*

	36	37	38	39
Years.	1842–3.	1871–2.	1850–1.	Mean of 1847, 48, 53.
No. of years observed ...	1	1	3	
Q { H =	....	0·006	0·012	0·010
Q { κ =	....	69	28	44
L { H =	0·095	0·016	0·006	0·009
L { κ =	135	110	280	255
N { H =	0·335	0·031	0·043	0·049
N { κ =	130	114	221	226
λ { H =	....	0·007	0·004	0·010
λ { κ =	....	72	190	308
ν { H =	....	0·003	0·003	0·011
ν { κ =	....	198	308	158
μ { H =	....	0·003	0·004	0·009
μ { κ =	....	73	187	193
R { H =				
R { κ =				
T { H =				
T { κ =				
MS { H =				
MS { κ =				
2SM { H =				
2SM { κ =				
Mm { H =	....	....	0·010	0·057
Mm { κ =	....	....	293	196
Mf { H =	....	....	0·019	0·061
Mf { κ =	....	....	229	159
MSf { H =	....	....	0·008	0·029
MSf { κ =	....	....	41	323
Sa { H =	....	....	0·151	0·123
Sa { κ =	....	....	185	254
Ssa { H =	....	....	0·170	0·108
Ssa { κ =	....	....	118	114

Table IV.

Penobscot Bay.

Year		1870.	1871.	1872.	1873.	1874.	1875.	Mean.
S_1	H =	0·068	0·015	0·022	0·020	0·020	0·002	0·024 ± 0·004
	κ =	129	78	67	123	73	25	65·9 ± 10·8
S_2	K =	0·825	0·735	0·776	0·797	0·746	0·747	0·771 ± 0·007
	κ =	350	356	357	354	354	358	354·7 ± 9·8
S_4	H =	0·008	0·004	0·003	0·006	0·005	0·004	
	κ =	113	73	346	222	350	29	
S_6	H =							
	κ =							
S_8	H =							
	κ =							
M_1	H =	Not reduced according to same rules as the rest of our results and omitted.						
	κ =							
M_2	H =	4·878	4·849	4·910	4·911	4·884	4·937	4·895 ± 0·008
	κ =	320	319	320	320	320	320	319·82 ± 0·10
M_3	H =	0·012	0·002	0·009	0·012	0·006	0·002	
	κ =	263	135	161	123	279	229	
M_4	H =	0·039	0·021	0·019	0·028	0·020	0·022	
	κ =	160	154	173	115	127	121	
M_6	H =	0·118	0·115	0·121	0·125	0·122	0·119	
	κ =	61	60	65	61	60	58	
M_8	H =	0·017	0·014	0·009	0·018	0·014	0·016	
	κ =	336	314	354	336	326	320	
O	H =	0·363	0·351	0·364	0·353	0·354	0·366	0·359 ± 0·002
	κ =	113	109	114	109	112	110	111·1 ± 0·55
K_1	H =	0·455	0·459	0·452	0·452	0·459	0·440	0·453 ± 0·002
	κ =	129	130	132	129	129	129	129·6 ± 0·35
K_2	H =	0·256	0·229	0·226	0·195	0·235	0·238	0·230 ± 0·006
	κ =	5	351	359	2	4	352	358·8 ± 1·7
P	H =	0·152	0·151	0·148	0·152	0·155	0·160	0·153 ± 0·001
	κ =	127	133	137	132	124	131	130·5 ± 1·2
J	H =	0·025	0·014	0·026	0·031	0·019	0·009	0·020
	κ =	341	266	320	315	323	292	315

Table IV.

Penobscot Bay.

Year ……		1870.	1871.	1872.	1873.	1874.	1875.	Mean.
Q	H =	0·060	0·058	0·073	0·058	0·077	0·073	0·066 ± 0·002
	κ =	245	271	259	246	272	284	262·8 ± 3·7
L	H =	0·336	0·172	0·195	0·285	0·223	0·209	0·237 ± 0·017
	κ =	190	187	156	193	219	209	192·1 ± 6·4
N	H =	1·046	1·136	0·986	0·929	0·991	1·027	1·019 ± 0·019
	κ =	295	291	287	289	291	289	290·3 ± 0·8
λ	H =	0·083	0·132	0·043	0·120	0·156	0·177	0·064
	κ =	162	256	146	256	125	234	196
ν	H =	0·162	0·335	0·093	0·320	0·215	0·289	0·274
	κ =	301	317	306	317	263	323	308
μ	H =	0·043	0·034	0·015	0·031	0·048	0·035	0·032
	κ =	237	194	176	241	219	202	216
R	H =	0·068	0·026	0·055	0·050	0·035	0·062	
	κ =	227	26	92	352	182	8	
T	H =	0·189	0·104	0·190	0·233	0·156	0·087	0·022
	κ =	239	139	50	331	243	123	288
MS	H =	0·036	0·016	0·025	0·010	0·028	0·010	
	κ =	308	210	21	299	311	75	
2SM	H =	0·039	0·014	0·021	0·025	0·055	0·025	
	κ =	297	135	318	270	327	41	
Mm	H =	0·082	0·069	0·024	0·074	0·057	0·014	
	κ =	96	35	34	296	279	110	
Mf	H =	0·012	0·037	0·048	0·047	0·073	0·040	
	κ =	1	208	11	212	4	95	
MSf	H =	0·190	0·157	0·163	0·229	0·180	0·123	0·174 ± 0·010
	κ =	161	151	152	161	159	159	157 ± 1·2
Sa	H =	0·080	0·176	….	0·120	0·177	0·262	κ is computed on hypothesis that these are astronomical tides.
	κ =	120	162	….	167	188	226	
Ssa	H =	0·090	0·093	….	0·097	0·026	0·152	
	κ =	48	57	….	111	146	74	

Table IV.

Port Townsend. *Astoria, Oregon.*

Commence, January, 1874.

Year	Port Townsend 1874.	1875.	1876.	Mean.	Astoria, Oregon 1874.	1875.	1876.	Mean.
S_1 H =	0·086	0·072	0·102	0·087	0·051	0·053	......	0·052
S_1 κ =	113	121	114	116·2	112	117	......	115
S_2 H =	0·557	0·558	0·542	0·552	0·778	0·774	0·811	0·788
S_2 κ =	130	129	129	129·5	39	38	41	40
S_4 H =	0·007	0·011	0·013	0·010	0·012	0·009	0·007	0·009
S_4 κ =	349	316	316	327	344	341	348	344
S_6 H =								
S_6 κ =								
S_8 H =								
S_8 κ =								
M_1 H =								
M_1 κ =								
M_2 H =	2·202	2·311	2·218	2·244	2·963	2·942	2·905	2·937
M_2 κ =	109	108	108	108·5	12	12	11	11·7
M_3 H =	0·021	0·015	0·022	0·019	0·021	0·013	0·029	0·021
M_3 κ =	41	343	298	347	107	63	34	68
M_4 H =	0·128	0·113	0·125	0·122	0·093	0·095	0·116	0·101
M_4 κ =	297	299	295	297	321	329	329	326
M_6 H =	0·032	0·027	0·028	0·029	0·033	0·026	0·033	0·031
M_6 κ =	240	255	236	244	121	115	111	116
M_8 H =								
M_8 κ =								
O H =	1·407	1·397	1·430	1·411	0·773	0·752	......	0·762
O κ =	132	131	130	131	119	118	......	118
K_1 H =	2·475	2·470	2·465	2·470	1·290	1·288	......	1·289
K_1 κ =	149	148	148	149	129	129	......	129
K_2 H =	0·171	0·145	0·167	0·161	0·233	0·214	......	0·224
K_2 κ =	128	132	137	132	24	27	......	26
P H =	0·776	0·751	0·787	0·771	0·374	0·347	......	0·360
P κ =	145	147	147	147	96	96	......	96
J H =	0·162	0·050	0·149		0·067	0·009		
J κ =	36	345	167		172	142		

Table IV.

Port Townsend. *Astoria, Oregon.*

Commence, January, 1874.

Year		1874.	1875.	1876.	Mean.	1874.	1875.	1876.	Mean.
Q	H =	0·297	0·315	0·295	0·302	0·175	0·156	......	0·166
	κ =	119	124	124	122	109	120	......	115
L	H =	0·085	0·107	0·080	0·091	0·117	0·119	0·109	0·112
	κ =	347	355	320	341	198	215	198	204
N	H =	0·461	0·466	0·440	0·456	0·574	0·556	0·543	0·559
	κ =	82	81	79	80	352	351	345	349
λ	H =	0·045	0·031	0·019	0·031	0·073	0·032	0·035	0·047
	κ =	6	29	332	2	192	200	150	181
ν	H =	0·156	0·089	0·029	0·091	0·202	0·127	0·129	0·153
	κ =	76	46	137	86	342	12	53	16
μ	H =	0·078	0·098	0·059	0·078	0·016	0·030	0·040	0·029
	κ =	352	7	356	358	130	142	108	127
R	H =	0·010	0·008	0·020	0·013	0·016	0·002	0·126	
	κ =	352	214	241	269	259	320	148	
T	H =	0·071	0·050	0·108		0·083	0·067	0·058	
	κ =	38	239	175		307	137	169	
MS	H =	0·062	0·072	0·058	0·064	0·055	0·049	0·053	0·052
	κ =	319	310	318	316	341	344	4	350
2SM	H =	0·011	0·017	0·018	0·016	0·018	0·021	0·030	0·023
	κ =	62	42	41	49	220	259	246	242
Mm	H =								
	κ =								
Mf	H =								
	κ =								
MSf	H =								
	κ =								
Sa	H =								
	κ =								
Ssa	H =								
	κ =								

Table IV.

	San Diego. Commence 0 h., January 1, 1869.				*St. Thomas.* Commence October 4, 1872.			
Year	1869.	1870.	1871.	Mean.	1872–3.	1873–4.	1874–5.	Mean
S_1 { H =	0·024	0·024	0·023	0·024	0·007	0·017	0·008	0·011
κ =	54	51	12	39	233	249	251	244
S_2 { H =	0·701	0·697	0·716	0·704	0·030	0·032	0·031	0·031
κ =	274	274	275	275	245	243	242	243
S_4 { H =	0·006	0·005	0·006	0·006				
κ =	221	169	204	207				
S_6 { H =								
κ =								
S_8 { H =								
κ =								
M_1 { H =								
κ =								
M_2 { H =	1·710	1·703	1·697	1·703	0·131	0·121	0·119	0·124
κ =	279	279	280	279	208	208	207	208
M_3 { H =	0·008	0·012	0·005	0·008				
κ =	32	67	48	49				
M_4 { H =	0·025	0·026	0·030	0·027				
κ =	200	193	194	196				
M_6 { H =	0·010	0·011	0·009	0·010				
κ =	150	118	110	126				
M_8 { H =								
κ =								
O { H =	0·697	0·698	0·714	0·703	0·237	0·240	0·253	0·243
κ =	71	71	72	71	149	153	156	153
K_1 { H =	1·010	1·010	1·010	1·010	0·290	0·296	0·300	0·295
κ =	96	96	96	96	173	170	170	171
K_2 { H =	0·207	0·202	0·194	0·201				
κ =	268	265	266	266				
P { H =	0·358	0·349	0·339	0·349	0·082	0·080	0·073	0·078
κ =	93	92	95	93	190	167	170	176
J { H =								
κ =								

Table IV.

San Diego. — Commence 0 h., January 1, 1869.

St. Thomas. — Commence October 4, 1872.

Year		1869.	1870.	1871.	Mean.	1872–3.	1873–4.	1874–5.	Mean.
Q	H =								
	κ =								
L	H =	0·065	0·028	0·032	0·042				
	κ =	62	29	117	69				
N	H =	0·423	0·412	0·401	0·412				
	κ =	262	263	264	263				
λ	H =								
	κ =								
ν	H =								
	κ =								
μ	H =	0·024	0·037	0·017	0·026				
	κ =	256	244	258	253				
R	H =								
	κ =								
T	H =								
	κ =								
MS	H =								
	κ =								
2SM	H =								
	κ =								
Mm	H =								
	κ =								
Mf	H =								
	κ =								
MSf	H =								
	κ =								
						Me	teorologi	cal	
Sa	H =	......	......	......	......	......	0·007	0·037	
	κ =	......	......	......	......	......	355	222	
Ssa	H =	......	......	......	......	......	0·049	0·061	
	κ =	......	......	......	......	......	98	207	

Table IV.

Sandy Hook.

Year	1876.	1877.	1878.	1879.	1880.	1881.	Mean.
S_1 H =	0·026	0·028	0·028	0·025	0·036	0·049	0·032
κ =	225	222	254	216	255	237	235
S_2 H =	0·439	0·432	0·436	0·445	0·416	0·435	0·434
κ =	246	245	248	245	242	249	246
S_4 H =	0·036	0·047	0·033	0·033	0·037	0·041	0·038
κ =	65	64	83	81	68	52	69
S_6 H =							
κ =							
S_8 H =							
κ =							
M_1 H =							
κ =							
M_2 H =	2·238	2·230	2·272	2·244	2·229	2·250	2·246
κ =	217	218	218	218	215	216	217
M_3 H =	0·025	0·022	0·021	0·035	0·029	0·030	0·027
κ =	191	196	202	192	222	206	202
M_4 H =	0·020	0·016	0·017	0·020	0·024	0·027	0·021
κ =	349	339	336	321	335	329	335
M_6 H =	0·049	0·048	0·053	0·046	0·057	0·059	0·052
κ =	352	355	351	344	344	342	348
M_8 H =							
κ =							
O H =	0·178	0·167	0·163	0·157	0·177	0·176	0·170
κ =	94	95	99	101	90	100	97
K_1 H =	0·322	0·330	0·340	0·337	0·333	0·342	0·334
κ =	91	91	90	91	88	90	90
K_2 H =	0·129	0·126	0·113	0·114	0·130	0·160	0·129
κ =	45	34	30	40	35	40	37
P H =	0·103	0·123	0·091	0·100	0·102	0·100	0·103
κ =	97	102	103	107	106	108	104
J H =	0·013	0·024	0·014	0·014	0·009	0·025	0·016
κ =	86	125	145	111	107	134	118

Table IV.

Sandy Hook.

Year		1876.	1877.	1878.	1879.	1880.	1881.	Mean.
Q	H =	0·039	0·039	0·029	0·033	0·033	0·037	0·035
	κ =	118	131	107	133	98	134	120
L	H =	0·103	0·110	0·108	0·084	0·075	0·072	0·092
	κ =	52	47	30	35	0	21	31
N	H =	0·470	0·507	0·532	0·500	0·457	0·475	0·490
	κ =	198	196	199	202	199	199	199
λ	H =	0·012	0·039	0·030	0·029	0·042	0·062	0·036
	κ =	15	26	26	69	60	13	35
ν	H =	0·045	0·124	0·167	0·153	0·065	0·077	0·105
	κ =	178	238	198	170	149	253	198
μ	H =	0·072	0·063	0·094	0·061	0·083	0·039	0·069
	κ =	221	216	235	207	249	236	227
R	H =	0·020	0·030	0·010	0·011	0·073	0·037	0·030
	κ =	324	241	19	16	318	9	334
T	H =	0·098	0·105	0·046	0·075	0·111	0·058	
	κ =	116	34	306	155	94	23	
MS	H =	0·045	0·037	0·050	0·039	0·041	0·040	0·042
	κ =	116	122	107	116	104	114	113
2SM	H =	0·018	0·014	0·007	0·021	0·010	0·005	
	κ =	138	158	66	237	338	323	
Mm	H =							
	κ =							
Mf	H =							
	κ =							
MSf	H =	0·030	0·014	0·010	0·042	0·011	0·014	
	κ =	41	171	332	224	230	23	
Sa	H =	0·083	0·066	0·066	0·072	0·060	0·058	0·068
	κ =	224	225	164	203	236	198	208
Ssa	H =							
	κ =							

November 19, 1885.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Mr. Cornelius O'Sullivan and Dr. Sydney Howard Vines were admitted into the Society.

Professor W. G. Adams, General Boileau, Dr. Huggins, Dr. Perkin, and Dr. Rae, having been nominated by the President, were by ballot elected Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read :—

I. "On the Total Solar Eclipse of September 9, 1885 (in a Letter to Professor STOKES, Sec. R.S.)." By JAMES HECTOR, M.D., F.R.S., Director of the Geological Survey, New Zealand. Received October 22, 1885.

Wellington, September 12, 1885.

DEAR SIR,

On the 5th instant I duly received your note of the 15th July, enclosing instructions drawn up by the Committee for observing the solar eclipse which took place here on the 9th.

The instructions were circulated in all quarters where they were likely to be of use. You will observe from the enclosures that unfortunately both the parties equipped by Government, in their anxiety to get as near the line of centrality as possible, encountered bad weather, so that no observations were made which can be considered

to have scientific exactitude. The eclipse, however, was very distinctly seen at Wellington, and I have been able, with the assistance of friends and the accounts in the newspapers, to obtain the following information concerning it.

Scarlet prominences were only moderately developed, and were clustered chiefly at the equatorial and polar regions of the sun. The best observers agree that the corona had a very irregular outline, and was most continuous and vivid close to the sun's limb, having the longest expansion reaching to nearly two diameters from the western equatorial region. This large expansion appears to have had a strongly marked spirally twisted structure, while all the other appendages consisted of radiating pyramids. No laminated structures appear to have been observed in any part of the corona.

Most observers agree in describing an intensely brilliant flash or meteor, lasting for two seconds, at the commencement of totality on the eastern side of the sun, and exactly over the position of a large sun-spot that was just coming into view at a few degrees south of the sun's equator. This flash is described as having looked like a large electric lamp suspended at a little distance from the moon's edge. At the close of totality another flash, similarly bright, but not so large and pointed, was seen on the western limb of the sun in a position corresponding with a large sun-spot that was within 1′ of arc of passing over the sun's edge.

The following is a list of the enclosures:—

1. Notification of the Committee's instructions.

2. Charts of shadow path showing the position of Dreyertown and Otahao.

3. General time plan of eclipse by Mr. Arthur Beverly.

4. Silver print of totality by M. Cazneau, Wellington.

5. Enlarged drawing from a negative $\frac{1}{8}''$ in diameter of the totality, photographed by Mr. Gell, Wellington.

6. Sketch by Mr. J. Buchanan, F.L.S.

7. Three sketches taken during totality at Wellington by Mr. T. W. Kirk. These were taken in succession as marked 1, 2, 3, the sun being intersected by a plumb-line, and disclose the remarkable feature that the N.W. extension of the corona shifted its position with reference to the prominence.

8. Generalised sketch from various sources, showing the outline of the corona, the position and shape of the prominences, and the positions, marked by red crosses, where vivid flashes of light were seen at the moment of beginning and end of totality. All agree that these flashes were like electric lights, and quite dazzling, the first being large and round, while the last seen was small and pointed.

9. Sketch of the large spirally twisted flame of the corona over the N.W. quadrant, as sketched by Mr. H. P. Higginson, C.E., with a

binocular glass. As Mr. Higginson observed and sketched the great eclipses of 1869 and 1870 in India, and is an accomplished draughtsman, he had previous experience to assist him.

10. Complete sketch by Mr. Higginson of the total eclipse finished from a drawing made with a binocular.

11. Sketch with the naked eye, by Mr. Alfred de Bathe Brandon, junr., which also shows the twisted character of the large coronal appendage.

12. Sketch showing the position of the sun-spots that were visible at the time of the eclipse, (*a*) being a very large spot that was just passing off, (*b*) a spot not visible before the eclipse, which has since developed into an intensely black sharply defined spot of moderate size.

13. Extracts from newspapers.

In conclusion, it is hardly necessary for me to state how much I regret that I am unable to give you fuller and more precise information founded on my own observation.

(Signed) J. Hector.

[The publication of a selection of the illustrations is deferred until some additional drawings or photographs expected from New Zealand shall have arrived. Enclosure No. 1, giving an account of a lecture delivered by Dr. Hector before the eclipse, is subjoined, with the omission of a portion at the beginning, the interest of which has now passed away.]

(*Extract from Enclosure No.* 1.)

The chief interest of the eclipse will lie in the observation of the scarlet prominences and of the silvery light of the corona or halo that surrounds the sun during the period of total darkness. The prominences may be expected to have great brilliancy, as for some weeks past the spots on the sun have shown that its surface is in a state of violent activity, and one of such unusual size as to be almost visible to the naked eye, will have reached such a position that it will coincide with the left hand edge of the sun at the time of the eclipse, and from this point unusually large flames should be looked for; for observing these flames it is necessary, in order to intensify their light, to use a fragment of rose-tinted glass. On the other hand, to observe the light of the corona to perfection, a very pale blue-tinted glass is necessary, so as to cut off the red light and intensify the pale silvery light of the corona. A telescope of high power is quite unsuitable to the observer, but a wide field opera glass will be useful. Instructions have been received from the Committee of the Royal Society, which relate partly to the taking of a photographic, spec-

troscopic, and other observations that require special instruments, but the following extracts may be of use to most observers :—Drawings of the corona have only seldom proved to be of great utility. If such drawings are attempted on the present occasion, observers ought to pay attention to the general outline of the corona rather than to points of detail. A plumb line ought to be suspended, if possible, between the observer and the sun, so as to fix the position of the corona in the sky as accurately as possible. The vertical line ought to be indicated on the drawing. Observers will find it useful to draw the black disk of the sun and the vertical line before the beginning of totality. . . . Observers unpractised in accurate drawing will obtain more useful results by paying attention to certain features of the corona than by attempting what can only be a very rough and inaccurate sketch of the corona. Definite answers as to the following questions, for instance, would be of great value :—(*a*) To what distance from the sun, estimated in solar diameters, can you trace the corona? (*b*) Does it extend further in some directions than in others, and what are the directions of greatest and least extent? (*c*) Is there a line of approximate symmetry in the corona, and what is the direction of that line? The answers to the last two questions ought, if possible, to be given in angles from the vertical line, or from some definite great circle.

II. "On the Total Solar Eclipse of September 9, 1885 (in a Letter to J. N. LOCKYER, F.R.S.)." By A. S. ATKINSON. Received November 19, 1885.

I observed the eclipse from a spot in my own ground in Nelson, which, as determined for the transit of Venus, is in lat. 41° 17′ 1·9″ S., and long. 173° 17′ 57·5″ E.

The sky was very clear, and there was no wind, but the air was optically very unsteady.

As totality was approaching, perhaps two or three minutes before, I tried with the telescope (5-in. Cooke, power 60) if I could see anything of the corona behind the moon, but could not in the time I allowed myself; I was afraid of waiting longer, as I had made arrangements for taking some small photographs, and had to superintend; and as I wished also to answer the questions of the Committee of the Royal Society, I thought it best to observe the main phase with the naked eye.

I may, perhaps, note here, that in finding my way with the telescope to the moon's following limb, I chanced upon Jupiter, the appearance

of which surprised me greatly. It was, of course, "boiling" a good deal, but at the moment I caught sight of it, it seemed to have one broad uniform equatorial belt, with at least its northern edge rather sharply marked; in breadth it seemed about one-third of the planet's (polar) diameter, and in colour distinctly pink. This belt disappeared and reappeared with the motion of the air. I shifted my eye in the telescope, but the breadth and colour seemed constant on each reappearance, so long as I looked, which was not, however, very long.

As the sun was just disappearing, the most striking phenomenon I noticed, looking straight at it, was a strongly marked pulsation in its light; those who were looking away from it saw waves of shadow passing rather rapidly over the ground. This also, I supposed, was from the unsteadiness of the air, but to me it seemed not the least striking part of the great spectacle to see the sun flickering as it were before it went out.

The following are my answers to the questions of the committee :—

a. I estimated the greatest distance from the moon's limb to which I could trace the corona as from two-thirds to three-fourths of a diameter.

b. The corona extended much farther in one direction than in any other. By far the greatest feature in the corona was a broad-based but hollow-sided cone of white light, with well-marked edges, and a rather sharp point, the axis of which I judged to be from 40° to 45° from the perpendicular towards the west. The "least extent" of the corona, as I saw it, was the same in several places, where there was only a narrow rim of light round the moon's limb. There were other smaller but more or less similar prominences of pure white light, all of which, I may say, gave me the idea of radiating from the sun's centre.

c. There was, in my opinion, no line of "approximate symmetry" in the corona. I looked right round the sun with a view to answer this question, and that was the conclusion I came to without hesitation. As there was nothing to balance the large "cone," the nearest approach to symmetry would have been obtained by taking its axis as the line, but I should not have called the result of this division "approximately symmetrical."

The only red prominences I saw were a row of six or seven small ones (Bailey's beads ?) extending from about the vertex towards the east. Large ones were seen by others, and I believe are those which alone appear in the photographs.

Mr. J. R. Akersten obtained for me two photographs during totality, one immediately after it began with an exposure of probably a little less than a second; the other a few seconds later, with about double the exposure. A third plate was all but ready when the sun

reappeared; it was taken just afterwards, but still shows some of the "red flames."

I took the duration of totality with a stop-watch, but afterwards by a momentary inadvertence lost the record.

I may add that at a time which I estimated to be from 15 to 20 seconds after the sun's reappearance, I could with the naked eye easily see the coronal light round the preceding limb of the moon, and called the attention of the bystanders to the fact.

In conclusion I would add as some evidence of the clear sky which we commonly get in Nelson, that from my own knowledge not only the whole of this eclipse, but egress in both the late transits of Venus, could not have been better seen than from this place.

(Signed) A. S. ATKINSON.

III. "Report on a Series of Specimens of the Deposits of the Nile Delta, obtained by the recent Boring Operations." By J. W. JUDD, F.R.S., SEC. G.S., Professor of Geology in the Normal School of Science and Royal School of Mines. Communicated by desire of the Delta Committee. Received November 12, 1885.

Neither of the borings made for the Royal Society, under the superintendence of the Engineers attached to the Army of Occupation in Egypt, appears to have reached the rocky floor of the Nile-Valley, nor do the samples examined show any indication of an approach to such floor. What were at first supposed to be pebbles in one of the samples from Tantah, prove on examination to be calcareous concretions ("race" or "kunkur").

Nevertheless these borings appear to have reached a greater depth than all previous ones in the same district, except the boring made near the Barrage, which is said to have attained a depth of 122 feet without reaching the rock, and one at Rosetta which exceeded 153 feet. The deepest boring made by the French engineers in 1799, that at Siut, attained a depth of 77 feet $7\frac{3}{4}$ inches; at a later date M. Linant de Bellefonds (Linant Bey) carried a boring near the apex of the Delta to the depth of 72 feet. In the case of the excavations made for Mr. Horner about thirty years ago, with the aid of a grant from the Donation Fund of the Royal Society, few of the borings exceeded 50 feet in depth; the deepest being that at Memphis, which reached 59 feet 10 inches. The three borings now reported upon have been carried to depths of 45, 73, and 84 feet respectively.

The samples from these borings, like those examined by Mr. Horner, show that the delta-deposits all consist of admixtures, in

varying proportions, of blown sand and alluvial mud. I can find no evidence to support the suggestion made by Sir J. W. Dawson, F.R.S. (see Colonel Maitland's letter of 21st January, 1884, and "Geol. Mag.," Dec. 3, vol. i, p. 292) from a hasty examination of the specimens, that "at a depth of 30 or 40 feet the alluvial mud rests on desert sand;" on the contrary these borings, like those of older date, show that the deposits of the Nile Valley consist of a succession of different beds in some of which sand, and in others mud, forms the predominant constituent.*

The chemical composition of the Nile-mud has been investigated by many chemists—by Regnault in 1812, by Lassaigne in 1844, by Lajonchère in 1850, by Payen and Poinset in 1850, and by Houzeau in 1869. At the request of Mr. Horner, Messrs. Johnson and Brazier undertook a series of careful analyses of the muds and the sands of the Nile-Valley deposits in Dr. Hofmann's laboratory, and the results which they obtained are published in the "Philosophical Transactions," vol. 145 (1855).

But hitherto, so far as I am aware, no detailed investigation of the microscopical characters of the deposits has been undertaken, and to this investigation I have therefore devoted my special attention. The methods of examination were determined upon after consultation with Professor T. G. Bonney, F.R.S., P.G.S., to whose advice and assistance I am greatly indebted; the preparations were made in the Geological Laboratory of the Normal School of Science and Royal School of Mines. The general results attained by this study of the Nile-Valley deposits are as follows.

The *sands*, when separated from the mud by washing, are found to be made up of two kinds of grains, the larger being perfectly rounded and polished, while the smaller, on the contrary, are often subangular or angular.

The larger and well-rounded grains may be described as microscopic pebbles; their surfaces are most exquisitely smoothed and polished, and their forms are either globular or ellipsoidal. In size they vary greatly, being occasionally as large as a small pea. They only very occasionally exhibit traces of deposits of iron-oxides upon their surfaces. The great majority of the well-rounded grains consist of quartzose materials.

* [The deep boring at Rosetta, of which an account has recently been sent to the Royal Society by Colonel Maitland, R.E., shows that alluvial mud and clay occurred to the depth of 33 feet; thence to the depth of 94 feet various sands and clays are recorded; from 94 feet to 123 feet "hard clay in lumps" was found; then various sands and clays to 143 feet; the last 10 feet of the boring was in coarse sand and pebbles. A fragment of red granite, with undoubted marks of human workmanship upon it, is said to have been obtained at a depth of 79 feet 4 inches.—J. W. J., 18th December, 1885.]

Embedding these grains in Canada-balsam, and examining them by transmitted light, with the aid of the polariscope, we are enabled to study their mineral characters with facility. The majority of the grains consist of colourless quartz, though occasionally rose-quartz, amethystine quartz, citrine, and smoky quartz also occur. This quartz exhibits unmistakable evidence of having been derived from granitic rocks; it is constantly seen to be traversed by bands of liquid- and gas-cavities, and very frequently contains numerous black, hair-like inclusions (rutile ?). Much more rarely we detect grains of quartz which consist of aggregates of small crystals, and are evidently derived from metamorphic rocks. With the pure quartz-grains we find also a considerable number of rounded particles of red and brown jasper and of black Lydian stone, with some fragments of silicified wood.

But in addition to the different varieties of quartz, particles of felspar are sometimes found among these large, rounded grains. What is very remarkable about these felspar-grains is the slight traces of kaolinization which they exhibit; they are in fact almost as fresh and unaltered as the grains of quartz themselves. Ordinary orthoclase and microcline are most frequent, while plagioclase felspar is comparatively rare. With the rounded grains of quartz and felspar, a few examples of hornblende and other minerals, including jade, also occur.

But far greater is the number of mineral species, especially the most easily cleavable ones, which are represented in the smaller, sub-angular and angular, sand-grains. In addition to the minerals already mentioned, I have recognised several varieties of mica, augite, enstatite ?, tourmaline, sphene, iolite (cordierite) zircon, fluorspar, and magnetite, all in a nearly unaltered condition.

The only fossils found in these sands were evidently derived ones; they include the fragments of silicified wood already mentioned, and a waterworn nucleus of an *Ammonites* (Jurassic ?). It is evident that these sand-grains have been formed by the breaking up of granitic and metamorphic rocks, or of older sandstones derived directly from such rocks. The larger grains exhibit the perfect rounding and polishing now recognised as characteristic of Æolian action;* the smaller ones from their larger surfaces in proportion to their weight have undergone far less attrition in their passage through the air; but it is fair to conclude that the sands are really "desert-sand," derived from the vast tracts which lie on either side of the Nile-Valley, and swept into it by the action of the wind.

* It is hardly necessary to point out that the study of these desert-sands entirely supports the important conclusions arrived at by Mr. H. C. Sorby, F.R.S., and Mr. J. A. Phillips, F.R.S., concerning the agencies by which the rounding of sand-grains is effected. See "Quart. Jour. Geol. Soc.," vol. 36 (1880). "Proceedings," p. 50. *Ibid.*, vol. 37 (1881), pp. 6—26.

The *mud* is a material of which it is much more difficult to study the mineral characters than the sand, owing to the extreme minuteness of its particles. It is a very striking fact, however, that kaolin, which constitutes the predominant constituent of clays, appears to be almost altogether absent from these Nile-muds. Chips and flakes of quartz, felspar, mica, hornblende, and other minerals can be readily recognised; and it is often evident that the unaltered particles of such minerals make up the greater part, if not the whole mass, of the fine-grained deposits. The mineral particles are of course mingled with a larger or smaller proportion of organic particles. Frustules of *Diatomaceæ* occur in these muds, as was pointed out by Ehrenberg, but unless special precautions were observed in collecting the samples it would be unsafe to draw any deductions from their presence.

In the case of the Tantah boring we find another kind of material besides the sand and mud. Almost all the samples from this boring contain angular fragments of an apparently tufaceous limestone. In addition to this we find minute sand-grains cemented by calcareous matter into small pellets of various sizes, and the particles of mud united in the same way are thus converted into argillo-calcareous concretions. In a sample taken from a depth of 70 feet in the Tantah boring, a number of irregular masses of this argillaceous limestone occurred; some of these were nearly 1 inch in diameter, and were at first supposed to be pebbles. On examination they were found to contain—

Calcic carbonate	77·3
Argillaceous matter	22·7
	100·0

There can be no doubt that sources of this calcic carbonate must exist near Tantah, probably in the form of calcareous springs.

The striking peculiarities of these sands and muds of the Nile Valley appear to be capable of a simple explanation. In countries where rain falls frequently and vegetation abounds, water charged with carbonic acid is constantly penetrating into the rocks and breaking up the compound silicates of which they are composed; the silicates of the alkalies and the alkaline earths being decomposed and their constituents removed in solution, while the silicate of alumina becomes hydrated and is carried away in suspension by water in the form of kaolin. In this way the felspars and nearly all other compound silicates are affected to such an extent that in most granitic and metamorphic rocks they show evidence of extensive "kaolinization," while the clays derived from them are made up for the most part of the crystalline plates of kaolin. But in dry and barren tracts, like portions of Northern Africa, none of these agencies will operate, and the

disintegration of the solid rocks is effected by mechanical means; the most potent of these mechanical agents of disintegration are the heat of the sun, causing the unequal expansion of the minerals which build up the rocks, the force of the wind, producing constant attrition of the disjoined particles, and torrential rains.

This being the case, it will be readily understood that the sand-grains will include felspar and other minerals in a nearly unaltered condition, while in countries where the chemical agents of the atmosphere come into play such particles would be more or less completely converted into kaolin. In the same way, the mud, instead of consisting of scales of kaolin originating from chemical action, will be formed of particles of the chemically unaltered minerals reduced to the finest dust by purely mechanical agencies.

The chemical analyses which have been made of these Nile-muds tend to support these conclusions. Instead of containing large quantities of combined water, as do all the ordinary clays, their composition is that of a mixture of anhydrous minerals. As the analyses hitherto made have none of them been undertaken with the object of estimating the exact proportions of hygroscopic and combined water in these muds, it would be well if some more careful determination of these two constituents could be made.

But there is fortunately a kind of evidence, derived from chemical analysis, which is of the greatest value from its bearing on the questions we are now discussing—that, namely, which is obtained from a study of the composition of the Nile-waters.

It must be remembered that the Nile is a river of a very peculiar and exceptional character. The last tributary which it receives is the Atbara, which falls into it in lat. 17° 38′ N.; from that point to its mouth, in 31° 25′ N. lat., the river does not receive a single affluent; for a distance of 1400 miles, indeed, it obtains no fresh supply of water except what is brought to it by superficial torrents after heavy rains in Lower Egypt. It has been clearly demonstrated that, after receiving the Atbara, the Nile undergoes a continual diminution in volume in its course through Egypt. This is no doubt in part due to percolation of the water through the delta-deposits, and in part to the water being drawn off in canals for purposes of irrigation; but a large part of this diminution in volume must certainly be ascribed to the great evaporation which must be going on from the surface of the river during the last 1400 miles of its course.

The diminution which takes place in the volume of the Nile in its downward course is illustrated by the following estimates:—

The French engineers in 1799 showed that at Siut, 240 miles above Cairo, the quantity of water passing down the river per second amounted to 678 cubic metres at low water, and 10,247 cubic metres during high water.

But at Cairo, according to M. Linant de Bellefonds (Linant Bey), the flow per second amounts only to 414 cubic metres at low water, and 9440 cubic metres at high water.

If these numbers can be relied upon, the Nile in this part of its course loses nearly 40 per cent. of its water in a distance of 240 miles when it is low, and nearly 8 per cent. when it is in flood!

M. Talabot has calculated that only 90,000 millions of cubic metres of water are discharged per annum from the principal mouth of the Nile, which gives an average discharge of 2680 cubic metres per second; while M. Girard estimates that only a comparatively small quantity escapes by the Rosetta and Damietta mouths.

Although we shall not be able to calculate the exact loss of the Nile by evaporation in the course of 1400 miles, through one of the hottest and driest regions of the globe, yet we cannot doubt that this loss is enormous. Now the effect of this constant evaporation must be to concentrate the saline matters held in solution, and we might therefore anticipate that the waters of the Nile in Lower Egypt would contain an exceptionally high percentage of saline matters in solution.

But what are the actual facts of the case?

Dr. C. Meymott Tidy has recently made a series of analyses of the Nile-water taken at Cairo during each month of the year, and the results which he has obtained are of the greatest interest to geologists. These analyses enable us to make the following comparisons, which we have arranged in tabular form*:—

	Filtered water from Nile.	Filtered water from Thames.	Filtered water from Lea.	Filtered water from Severn.	Filtered water from Shannon.
Total solids in grs. per gallon	9·53 to 14·33	18·24 to 21·63	17·99 to 23·34	13·95 to 22·75	15·30 to 20·20
Proportion of lime ,,	2·05 to 4·35	6·89 to 8·74	6·16 to 9·28	4·03 to 5·43	5·82 to 7·28
Hardness before boiling	5·0 to 8·0	12·9 to 14·3	13·0 to 15·9	10·0 to 14·3	9·5 to 12·5
Hardness after boiling	1·2 to 2·8	3·1 to 4·2	3·3 to 4·4	5·0 to 9·5	3·5 to 4·7

* "Journal of the Chemical Society," vol. xxxvii, 1880, "Transactions," pp. 268—327.

The examination of this table shows that the waters of the Nile, judged either by the proportion of solids in solution, the percentage of lime, or by the temporary and permanent hardness, exhibits some very remarkable anomalies. That the variation in the quantities of dissolved salts at different seasons of the year should be much greater than in the case of any of the other rivers, is not surprising when we bear in mind the great and rapid additions to the volume of the river during seasons of flood. But it is startling to find that the water of the Nile, instead of containing a much larger proportion of saline matter than other rivers—as we might anticipate from the enormous evaporation constantly going on from its surface—in reality contains far less dissolved matter than any of the other rivers here compared with it. This contrast is scarcely less striking in the case of the comparison with the Severn and the Shannon, which flow over Palæozoic rocks, than in that of the Thames and the Lea, which drain areas occupied by Mesozoic deposits.

A little consideration will show, however, that this startling and seemingly anomalous result is capable of a very simple explanation. The substances dissolved in the water of rivers is of course derived from the materials composing the rocks of the river-basin, through the action of water holding carbonic acid or other acids in solution. In this kind of action, rain which takes up the gases contained in the atmosphere, and then percolates into the interstices of rocks, plays a most important part. Rain is, in fact, the great agent of chemical disintegration, and where rain falls the complex silicates composing the hardest rocks are attacked, the silicates of potash, soda, lime, magnesia, and iron being broken up and carried away in solution, while the silicate of alumina takes its hydrated form of kaolin, and remains behind or is removed in suspension.

But in districts where there is little or no vegetation and the rainfall is sudden and torrential, this chemical disintegration, as has been already pointed out, is replaced by totally different kinds of action. Under the influence of variations of temperature, having an enormous range, the rocks made up of crystals having different coefficients of expansion, in different minerals and indeed in different directions in the same crystal, undergo mechanical disintegration, and the fragments thus formed are driven backwards and forwards by wind, being thereby subjected to constant attrition. Of this latter kind of action, as we have seen, all the larger particles in the Nile Delta deposits exhibit the most unmistakable evidence.

Hence we are led by an examination of the composition of the Nile-water to the same conclusion as was reached by the study of microscopical characters of the muds and sands of the delta, that while in the rainy districts of the temperate zones, the disintegration of rocks is mainly effected by chemical agencies, in the desert areas

of the tropics the same work is almost exclusively effected by mechanical forces.

The products of these two kinds of action are, however, essentially different. In the former case we have produced crystals of kaolin, which form the basis of all the true clays, a large quantity of lime-, magnesia-, iron-, soda-, and potash-salts with silica passing into solution; while, in the latter case, the several minerals of the rock are simply reduced to fragments of varying size and form.

All the observations described in the present report are in entire harmony with this explanation. The comparatively unaltered condition of the felspars and other complex silicates in the sands; the absence of kaolin from the muds, and the presence of the chips and flakes of the unattacked minerals in the muds, and finally the small quantity of dissolved matter in the Nile-water, in spite of the enormous concentration it must have undergone by evaporation—all point to this same conclusion.

In the estimates which have been made of the rate of subaerial denudation in different parts of the globe, it has usually been assumed that this action is similar to what is seen taking place in Europe and in North America. But the observations detailed in this report prove that in tropical districts, where little or no vegetation exists, and the rainfall is sudden and torrential, the disintegration of rocks, though not, perhaps, less rapid than in temperate climes, is different alike in its origin and in its products.

It has often been pointed out by chemical geologists, that metamorphic action could not have produced many of the schists and gneisses from sedimentary rocks, for the former are rich in potash, soda, and other materials which have been dissolved out from the latter during the disintegration of the rock-masses from which they were derived. The recognition of a kind of action whereby great masses of sedimentary materials can be produced, rich in those substances which are usually removed in a state of solution, is not destitute of interest at the present time, when the question of the origin of the crystalline schists and gneisses is one that presses for solution.

Appendix.

Examination of the Samples obtained during the Recent Borings in Egypt.

Portions of the several samples, varying in weight from 8 to 18 grams, were dried at 110° C., and then weighed. These portions were, by careful levigation, separated into mud and sand, which being dried at 110° C. and weighed, afforded the necessary data for calculating the percentage composition of each sample.

One specimen of each sample of sand thus obtained was mounted

dry for study by reflected light; a second specimen from each was mounted in Canada-balsam, so as to allow of the internal structure and optical properties of the sand-grains being studied.

The fine particles composing the several samples of mud were so mounted as to be capable of examination by transmitted light, with the highest powers of the microscope.

The results obtained in the case of each sample were as follows :—

I. *Boring at Kasr-el-Nil, Cairo.*

Sample 1. Depth, 6 ft.

Loam of a dark reddish-brown colour. Contains 5·77 per cent. sand, 94·33 per cent. mud.

Sand.—Dark coloured; includes fragments of red brick, green glass, straw, rootlets, and other vegetable matters. Rounded grains of white quartz, evidently derived from granitic rocks, with a few of jasper and rose-quartz; angular or subangular grains of quartz, hornblende, &c., with many of fine-grained, impure sandstone.

Mud.—Much vegetable matter, with frustules of diatoms. Minute chips and flakes of quartz and other minerals, with plates of biotite.

Sample 2. Depth, 16 ft.

Very sandy loam, of pale reddish-brown tint. Contains 86·27 per cent. sand, and 13·73 per cent. mud.

Sand, composed of about equal portions of well-rounded and angular or subangular grains. It is of a white or slightly yellowish tint. The rounded grains consist of white (granitic) quartz, rose-quartz, and red, brown, and black jasper; with pebbles of slightly kaolinised orthoclase felspar. The angular and subangular grains consist of quartz, felspar, hornblende, sphene, and magnetite.

Mud.—Appears to be largely made up of chips and flakes of the above minerals with biotite.

Sample 3. Depth, 17 ft. 6 in.

Very sandy loam. Consists of 79·65 per cent. sand, 20·35 per cent. mud.

Sand.—Very similar to last sample, but the well-rounded grains are fewer, and the angular and subangular ones more numerous. The latter consist of quartz, orthoclase, plagioclase-felspar (rare), hornblende, augite; also black and brown jasper.

Mud.—Similar to last sample.

Sample 4. Depth, 38 ft. 6 in.

Sandy loam. Consists of 65·05 per cent. sand, and 34·95 per cent. mud.

Sand.—Mostly angular or subangular. Only a few well-rounded grains. Consists of quartz (granitic), with a few particles derived from metamorphic rocks. Microcline and orthoclase, but only very slightly kaolinised; plagioclase-felspar, hornblende, augite (?), sphene (?), magnetite; with red, brown, and black jasper.

Mud.—As in last.

SAMPLE 5. Depth, 40 ft. 6 in.

Sandy loam. Consists of 80·83 per cent. sand, 19·17 per cent. mud.

Sand.—Consists of about equal proportions of rounded and of angular or subangular grains. The materials are quartz, with fine inclusions of rutile, apatite (?), &c., with bands of liquid and gas cavities, microcline and orthoclase (slightly kaolinised), plagioclase-felspar, hornblende, red, brown, and black jasper, glauconite (?) grains.

Mud.—As in last.

SAMPLE 6. Depth, 45 ft.

Sandy loam. Consists of 68·72 per cent. sand, 31·28 per cent. mud.

Sand.—The angular and subangular particles preponderate over the rounded ones. The minerals represented are quartz (granitic), with hair-like inclusions (rutile), quartz of metamorphic origin, microcline and orthoclase (slightly kaolinised), plagioclase-felspar, hornblende, and biotite. Also red, black, brown, and green jasper.

Mud.—As in last.

II. *Boring at Kafr-ez-Zayat.*

SAMPLE 1. Depth, 3 ft.

Mould of yellowish-brown colour. Consists of 2·35 per cent. sand, and 97·65 mud.

Sand.—Mostly angular or subangular, but a few well-rounded grains present. Consists of grains of quartz (granitic), with many acicular crystals inclosed, and bands of cavities. Fragments of hornblende abound. Orthoclase and a little plagioclase-felspar, jasper, and small calcareous concretions. Vegetable matter.

Mud.—Contains much organic matter. Frustules of diatoms.

SAMPLE 2. Depth, 4 ft.

Light coloured, sandy loam. Consists of 30·42 per cent. sand, 69·58 per cent. mud.

Sand.—About an equal admixture of rounded with angular or

subangular grains. Consists of the usual minerals, but mica (biotite) in thin brown plates occurs in tolerable abundance.

Mud.—Presenting the usual characters.

SAMPLE 3. Depth, 11 ft.

Pale coloured, very sandy loam. Consists of 50·99 per cent. sand, 49·01 per cent. mud.

Sand.—Mostly angular or subangular, with a few well-rounded grains. The usual minerals, with smoky quartz, tourmaline, and sphene (?). Fragments of silicified wood.

Mud.—Presents the usual characters.

SAMPLE 4. Depth, 19 ft.

Coarse sand of ash-grey colour. Consists of 87·41 per cent. sand, and 12·59 per cent. mud.

Sand.—For the most part made up of large, well-rounded grains, nearly all of which are colourless quartz (granitic), with a few of jasper and felspar (including orthoclase, microcline, and plagioclase). Also a few calcareous concretions. The smaller sand grains are sometimes angular, and sometimes subangular; they consist of the usual minerals, including both hornblende and mica.

Mud.—Consists almost entirely of inorganic particles.

SAMPLE 5. Depth, 26 ft.

Very coarse, nearly white sand. Consists of 90·19 per cent. sand, 9·81 per cent. mud.

Sand.—The coarser grains are nearly all well-rounded, and are, in fact, converted into beautiful pebbles. These larger grains are usually quartz (granitic), but well-rounded fragments of hornblende and felspar also occur, with red, yellow, and brown jasper by no means rarely. The smaller grains, which are usually angular or subangular, include a greater variety of materials.

Mud.—Composed almost wholly of mineral particles.

SAMPLE 6. Depth, 35 ft.

Sand similar to last, but finer grained. Consists of 86·42 per cent. sand, 13·58 per cent. mud.

Sand.—The coarser particles usually well-rounded and composed of quartz (granitic), with numerous and large liquid inclusions, with a little felspar and hornblende. There occurred in this sample a calcareous fragment which appeared to be the nucleus of an *Ammonites* (Jurassic ?). The finer sand, which is angular or subangular, consists of the usual minerals, the quartz of metamorphic rocks being commoner than is usually the case.

Mud.—Of usual character.

SAMPLE 7. Depth, 40 ft.

Dark coloured sand. Consists of 81·94 per cent. sand, and 18·06 per cent. mud.

Sand.—Composed of very different sized particles; the coarser being nearly always well-rounded pebbles of colourless quartz, with some jasper. A large proportion of the finer sand-grains in this sample are also well rounded.

Mud.—Of usual character.

SAMPLE 8. Depth, 48 ft.

Lighter coloured sand. Consists of 87·23 per cent. sand, and 12·77 per cent. mud.

Sand.—Varies greatly in coarseness, many of the smaller grains being well rounded. The rounded grains are almost all colourless quartz (granitic), but smoky quartz, rose-quartz, citrine, and jasper, are not rare. The finer sand consists of the usual minerals.

Mud.—Usual characters.

SAMPLE 9. Depth, 55 ft.

Dark coloured mud. Consists of 0·25 per cent. sand, and 99·75 per cent. clay.

Sand.—Fine grained, and composed in great part of calcareous concretions of the muddy particles. Most of the other grains are angular, and consist of the usual minerals; well-rounded quartz grains are very rare in this sample.

Mud.—Consisting of coarser particles than usual, is seen to be made up of quartz, felspar, hornblende, and mica fragments, and kaolin particles are rare.

SAMPLE 10. Depth, 60 ft.

Dark coloured sand, clay, or mud. Consists of 12·60 per cent. sand, and 87·40 per cent. mud.

Sand.—Mostly well-rounded grains of quartz, with hornblende and calcareous concretions by no means rare. Grains of black carbonised vegetable matter.

Mud.—Contains much organic matter and some kaolin particles, with the same mineral fragments as in the last sample.

SAMPLE 11. Depth, 66 ft.

Dark coloured very sandy clay. Consists of 62·07 per cent. sand, and 37·93 per cent. mud.

Sand.—Mostly fine grained, and the particles generally angular. A few well-rounded quartz-particles; plagioclase and orthoclase felspar, hornblende, &c., also occur. Several angular and subangular frag-

ments of a clear glass exhibiting strain-phenomena by polarised light —probably of artificial origin.

Mud.—Almost wholly composed of very fine-grained, inorganic particles.

SAMPLE 12. Depth, 75 ft.

Lighter coloured, sandy clay. Consists of 66·38 per cent. sand, and 36·62 per cent. mud.

Sand.—Of very different degrees of fineness, the coarser grains being nearly all well rounded. The usual minerals are seen, including very fine microcline and large pieces of muscovite.

Mud.—Consists of inorganic particles, none very fine. Quartz, felspar, and hornblende can be recognised among the larger ones.

III. *Boring at Tantah.*

SAMPLE 1. Depth, 4 ft.

Dark coloured mould. Consists of 1·71 per cent. sand, and 98·29 per cent. mud.

Sand.—Generally angular or subangular, with some rounded grains. Many fragments of brick, and some splinters of green glass. Many calcareous concretions. Much carbonaceous matter.

Mud.—Consists principally of organic matter, with some mineral fragments.

SAMPLE 2. Depth, 8 ft. 6 in.

Hard brown clay. Consists of 7·27 per cent. sand, and 92·73 per cent. mud.

Sand.—Contains both coarse, rounded grains and finer, angular or subangular ones. Quartz, hornblende, and the other minerals usually present are represented. Pale coloured granular calcareous particles. Dark coloured argillo-calcareous particles (like those represented on a large scale at greater depths).

Mud.—Generally very fine grained, with some organic particles.

SAMPLE 3. Depth, 18 ft.

Hard brownish clay. Consists of 8·78 per cent. sand, and 91·22 per cent. mud.

Sand.—Made up of granular calcareous particles, with only a few grains of sand, rounded or angular, consisting of quartz and the usual minerals. One fragment of red brick, and a number of particles of black slag, full of bubbles (resembling slag of Roman iron-furnaces).

Mud.—Contains some coarse particles of different minerals.

SAMPLE 4. Depth, 22 ft. 6 in.

Dark coloured loam. Consists of 31·56 per cent. sand, and 68·44 per cent. mud.

Sand.—Consists of an admixture of rounded subangular and angular grains of the usual minerals. A smaller admixture of calcareous particles of both types.

Mud.—Consists largely of fine fragments of the usual minerals.

SAMPLE 5. Depth, 31 ft.

Dark coloured sandy loam. Consists of 39·43 per cent. sand, and 60·57 per cent. mud.

Sand.—Made up of fine-grained materials, in which subangular fragments predominate; they consist of the usual minerals, with a few light coloured calcareous concretions.

Mud.—Consists of coarse particles of the usual minerals.

SAMPLE 6. Depth, 40 ft.

Dark coloured sand. Consists of 80·70 per cent. sand, and 19·30 per cent. mud.

Sand.—Much coarser than usual, the particles mostly well rounded; they may be called, indeed, quartz pebbles. The usual minerals are represented, with the addition of zircon and enstatite (?). Calcareous concretions and a derived fossil (obscure).

Mud.—Very fine grained.

SAMPLE 7. Depth, 46 ft.

Lighter coloured very coarse sand. Consists of 95·90 per cent. sand, 4·10 per cent. mud.

Sand.—Made up of coarse grains, all well rounded and polished. Milky quartz and amethyst recognised. Only a small quantity of angular and subangulur fragments. Calcareous concretions present.

Mud.—Very fine grained.

SAMPLE 8. Depth, 55 ft.

Very coarse sand. Consists of 97·71 per cent. sand, and 2·29 per cent. mud.

Sand.—Made up of coarse particles of different minerals, all well rounded; some angular fragments of limestone. Among the rounded particles the following minerals were detected: colourless (granitic) quartz, citrine, jasper, milky and rose quartz, iolite (cordierite), tourmaline, nephrite (?).

Mud.—Small in quantity, and very fine grained.

Sample 9. Depth, 56 ft.

Excessively coarse-grained sand. Consists of 99·53 per cent. sand, and 0·47 per cent. mud.

Sand.—Made up of large well-rounded grains of quartz of all colours, with jasper, &c. Angular fragments of limestone of a tufaceous character of considerable size. Fluorspar. One piece of *jade* which did not show signs of being worked.

Mud.—Very fine grained.

Sample 10. Depth, 58 ft.

Reddish-brown sandy loam. Consists of 59·09 per cent. sand, and 40·91 per cent. mud.

Sand.—Coarse well-rounded particles, with an admixture of angular ones. Among rounded particles, several of jade. Other minerals as usual, with black jasper or Lydian stone, tourmaline, fluorspar. Angular fragments of tufaceous limestone, containing sand grains.

Mud.—Very fine grained, with some organic matter.

Sample 11. Depth, 68 ft.

Hard reddish-brown clay, with some calcareo-argillaceous concretions. Consists of 7·76 per cent. sand, and 92·24 per cent. mud.

Sand.—Admixture of large well-rounded particles of quartz and smaller subangular ones of different minerals, with irregular calcareo-argillaceous grains.

Mud.—Made up of coarse particles of different minerals.

Sample 12. Depth, 73 ft.

Ash-grey sandy loam. Consists of 59·95 per cent. sand, and 40·05 per cent. clay.

Sand.—Rather fine, and mostly subangular or angular, with some large and well-rounded grains. The usual minerals are present, including amethyst and milky quartz.

Mud.—Rather coarse grained, and consisting of recognisable mineral particles.

The samples, with the preparations made from them, have, by the desire of the Delta Committee of the Royal Society, been deposited in the British Museum, Natural History, Cromwell Road, South Kensington.

IV. "On Evaporation and Dissociation. Part I." By Professor WILLIAM RAMSAY, Ph.D., and SYDNEY YOUNG, D.Sc., Lecturer and Demonstrator of Chemistry in University College, Bristol. Communicated by Professor STOKES, Sec. R.S. Received August 4, 1885.

(Abstract.)

The authors describe experiments made with the object of ascertaining whether the coincidence of the curves which represent the vapour-pressures of stable solid and liquid substances at different temperatures, with those indicating the maximum temperatures attainable by the same substances at different pressures, when evaporating with a free surface, holds good also for bodies which dissociate in their passage to the gaseous state. The substances examined were chloral hydrate, chloral methyl, and ethyl alcoholates, butyl chloral hydrate, ammonium carbonate, ammonium chloride, aldehyde ammonia, phthalic acid, succinic acid, nitric peroxide, and acetic acid.

These substances fall into two classes, the first including bodies giving coincident curves, viz., nitric peroxide, ammonium chloride, and acetic acid, the second containing the remaining substances, which do not show such coincidence. Comparing the members of the second class with each other as regards temperatures of volatilisation, it is found that in those cases in which dissociation is complete, or nearly so, the temperatures of volatilisation are independent of pressure and do not form a curve. When dissociation is less complete, as with succinic and phthalic acids, a rudimentary curve is observable, and with aldehyde ammonia, which is much more stable, the temperatures of volatilisation form a regular curve resembling a vapour-pressure curve.

All those substances, however, give curves representing pressures of dissociation, generally similar in form to vapour-pressure curves, and a comparison of these curves with those representing temperatures of volatilisation would indicate that the smaller the amount of dissociation the nearer the curves approach each other both in form and position.

It is noticeable that in the formation of bodies of the second class a molecule of water or ammonia is invariably broken down, whereas with nitric peroxide and acetic acid direct union of like molecules takes place, and there is no such rupture as in the previous cases. As ammonium chloride resembles these substances in the likeness of their behaviour to that of stable solids and liquids, it may perhaps be

conjectured that the molecule of hydrogen chloride is not broken down in its union with ammonia to form ammonium chloride. Should this conjecture not be accepted as correct, it will be necessary to seek for an explanation of the phenomena observed by some relations yet to be discovered.

V. "On the Phenomena accompanying Stimulation of the Gland-Cells in the Tentacles of *Drosera dichotoma.*" By WALTER GARDINER, M.A., Fellow of Clare College, Cambridge, Demonstrator of Botany in the University. Communicated by Professor M. FOSTER, Sec. R.S. Received September 5, 1885.

(Preliminary Communication.)

Method of Research.—Pieces of unstimulated leaves, and of leaves stimulated for periods varying from 5 minutes to 72 hours, were examined fresh, or after treatment with alcohol, picric acid, chromic acid, or osmic acid. The most satisfactory results were obtained from specimens treated for 12 hours with 1 and 2 per cent. chromic acid; such strengths dissolving the tannin precipitate first formed, and fixing the structures most successfully. The leaves were fed principally upon small flies or pieces of frog muscle, since these were found to succeed best. Heat stimuli, electrical stimuli, and stimulus produced by contact or cutting were also employed.

General Histology.—As regards the general histology of the tentacles, one may notice that the gland-cells of the head are provided with delicate uncuticularised cell-walls, which are remarkably pitted on their upper or free surfaces; that the rest of the epidermal cells of the tentacles have their exterior walls excessively cuticularised and resistant, and that their radial longitudinal walls are freely pitted.

Structure of the Resting Gland-Cells.—In the typical resting gland-cell, the protoplasm is arranged in a network or reticulum. The meshes of this reticulum are excessively close around the nucleus, which is situated at the base of the cell, but towards the free surface they are much more open; the close and more open arrangement merging the one into the other. The meshwork extends through the whole of the cell cavity, and the interstices between the meshes are occupied by the pink cell sap; the whole being bounded by the ectoplasm. The gland-cells at the base of the head differ somewhat in structure from the more apical cells, as also do all the cells of the short stalked tentacles which are situated at the centre of the leaf. In neither of the three layers of cells covering the tracheïdal cells of the head could any obvious movement of the protoplasm be detected.

The epidermal cells of the stalk of the tentacles possess in the resting condition the following structure. In each cell there is a lenticular nucleus, and chlorophyll grains are present on the side of the wall next the vascular bundle. These structures are situated in the ectoplasm, and take no part in the movement of rotation. In addition to these a body, which is usually spindle-shaped or acicular, is present in the cell, and generally occupies such a position that it stretches diagonally across the cell from end to end, the two extremities being embedded in the cell protoplasm. I shall speak of this body at present as the *plastoid*, on account of certain resemblances it bears as regards its microchemical reactions to plastids. This name may possibly, however, not be retained. The plastoid is fixed to some extent by absolute alcohol or chromic acid. With dilute alcohol it swells up and disappears. With iodine it becomes disorganised and spherical. It is best fixed by watery picric acid, and stains very readily and at once with Hofmann's blue. In the resting stage the plastoid takes no part in the movements of the rotating endoplasm. It is present in all the epidermal cells of the leaf except the gland-cells and the cells immediately beneath the same, and in the bending portion of those tentacles which execute movements, it is very large in the cells of the convex, and very small or even apparently absent in those of the concave side. In Dionæa it also occurs, being large in the cells of the upper surface of the leaf, and very small in those of the under. The cells themselves of the under surface in Dionæa and of the concave side in Drosera are also smaller than the others of the opposite side. The protoplasm of the tentacle cells is very clear and hyaline, and the whole protoplasmic utricle is thin and closely pressed against the cell-wall.

Changes in the Gland-Cells during Secretion.—The cells may be made to secrete by the combined stimulus of heat and moisture, by direct contact, or by electrical stimulus, but especially by the stimulus of food applied to the gland.

The histological changes which occur are the following. After some time (24 hours) a gland mounted in water exhibits a mottled appearance, such mottling being caused by a vacuolation of the most peripheral portions of the protoplasm of the gland-cells. In section such a cell shows, that in the course of secretion there has been a using up of the cell contents, and instead of the meshwork occupying the whole of the peripheral portion of the cell, so as to give a fairly homogeneous appearance, large spherical cavities have appeared in the reticulum here and there: such cavities being occupied by the cell sap. The sap has, moreover, assumed a much darker pink tint. Thus a breaking down or destruction of some part of the reticulum has taken place. After some 72 hours' stimulation this breaking down of the reticulum has reached to such an extent that in the peripheral portion

before referred to, all the central core of the meshwork has for the most part disappeared, and replacing it is a single large vacuole filled with cell sap. The ectoplasm has moreover contracted from the upper or free surface of the cell-wall. In no case does this destruction and consequent vacuolation extend to the base of the gland-cells where the nucleus is situated. The nucleus is always surrounded by dense protoplasm; and there are grounds for believing that after very long stimulation, when all the secretion has been poured out, and before absorption begins, an active growth of protoplasm takes place around the nucleus and in the more basal portion of the cell. In certain cases the secretion can be seen under the microscope to escape in drops—apparently through the pitted portions of the cell-wall—and the drops rapidly taking up water and being forced outwards, assume a rod-like form, and present a halo-like appearance around the gland. This also occurs in the mucilage-secreting cells of the bladders of Utricularia. The exact part taken by the layer of cells beneath the gland-cells has yet to be determined. After stimulation their vacuoles are occupied by large drops of cell sap, which is of a purple or even black colour.

The view here taken (which is supported by certain of the staining reactions) with regard to secretion is, that in the gland-cells the mere peripheral network consists of protoplasm, together with some formed substance derived from it, and that the outpouring of the secretion is caused by the repeated breaking down (owing to stimulation) of the protoplasm into this formed substance, which is of a mucous nature, and which rapidly attracts water and so escapes, as the secretion to the external surface.

Changes in the Stalk Cells.—The chief phenomena induced in the stalk cells either by contact by electrical stimulus or by feeding are, that the protoplasmic utricle swells up and encroaches on its own vacuole, that granules rapidly appear in the protoplasm, and that the movements of rotation increase in vigour. Also the cell becomes less turgid, and after long stimulation the plastoid and the nucleus both tend to become spherical. These changes are most markedly exhibited in tentacles stimulated with food. The protoplasm in swelling up abstracts water from its own vacuole, and in so doing leaves the tannin in the sap, in a comparatively concentrated condition. The outlines of the protoplasmic utricle are now rendered clearer than before. As previously stated, the movements of rotation become quickened very considerably, and numerous waves with high crests appear on the surface of the protoplasmic utricle, and are well registered by the corresponding disturbances in the cell sap. The phenomenon of the protoplasmic waves breaking over the nucleus with crests reaching nearly across the vacuole is very remarkable. The long and narrow shape of the cells, and the combined swelling of the

protoplasm, increase of rate of rotation and wave movements, cause the cell sap to be so disturbed, and as it were churned up, that drops of the sap become cut off from the main mass, and at last the whole of the sap is separated into numerous distinct portions which are suspended in the protoplasm as oily drops, and are carried round in the currents, presenting the appearance of moving droplets, pear-shaped bodies, and long string-like processes. The cell in this condition was known to Darwin as "aggregated." When movement ceases, as often naturally occurs, owing to excessive secretion, or can be induced by suddenly crushing the tentacle head, the variously shaped masses become spherical, and lie quiescent in the protoplasm. The aggregation produced by the action of ammonic carbonate is somewhat different from that brought about by feeding, and may be spoken of as passive as opposed to active aggregation. In this case the protoplasm abstracts water from the vacuole in the usual way, and, steadily swelling, chops up the tannin-loaded sap of the long and narrow cells into separate globules. The rotatory movements cause these globules to alter their form, but the movements in question are nothing like so vigorous as in the food-stimulated gland, and the globules are rarely, if ever, carried bodily about.

When the protoplasm swells and the cell becomes aggregated, the latter always loses its turgidity, and the state of aggregation is accompanied by a loss of water. Injection of water into the tissue will at once stop aggregation and restore the cell to its normal condition. Sometimes after active secretion (48 hours), the movements of the protoplasm of the topmost tentacle stalk cells will stop, and much of the protoplasm of each cell now collects to the end of the cell nearest the gland. Mounting in water will then restore the movements. It was found experimentally that the collecting of the protoplasm to one end was occasioned by the upward passage of water to the gland. If a passage of water be set up in an opposite direction, then the protoplasm collects to the opposite end.

When the more rapid movements of the protoplasm commence owing to stimulation, the plastoid usually becomes bent, and then either contracts and assumes a lenticular form, or becomes separated into two or more pieces each of which becomes lenticular. Later on further contraction ensues, and the plastoid is carried round the cell in the protoplasmic stream. The more the cells lose their turgidity, the more does the plastoid tend to assume a spherical form. Its spindle-shaped elongated form may, however, be restored by again bringing about turgidity, *e.g.*, by injection of water into the tissue. Thus the plastoid may be regarded as a turgometer, since it indicates the state of turgidity of the cell. On account of certain experiments and observations the author is led to believe that all differences of turgidity in cells are brought about by the protoplasm swelling and

becoming porous, and that the method of establishing a loss of turgidity by solutions of neutral salts and the like involves a state of things essentially different from that which normally occurs. In the one case the protoplasm itself undergoes change. In the other the cell sap is violently abstracted by artificial means from the vacuole of a protoplasmic utricle which is endeavouring to protect itself from the action of the reagent.

Movements of the tentacles may be brought about by direct contact and by cutting or injury, by electric stimulus, by the addition or withdrawal of water, and especially by the stimulus of food. In all these cases the stimulant upsets the existing equilibrium and brings about a difference of turgidity on the two opposite sides. Loss of water by plasmolysis usually induces movement, unless the salts employed have a specific action upon the protoplasm. Movement may occur without aggregation, and secretion may occur without movement, but whenever well-defined aggregation takes place, movement always follows. Different strengths of the same reagent may bring about different reactions. Thus a 0·1 per cent. solution of chromic acid causes both movement and secretion, while with a 1 or 2 per cent. solution neither phenomenon occurs. Among the curious effects of various salt solutions that of ammonic chloride may be noted, in that it acts markedly as a sedative, toning down aggregation and restoring turgidity. It was very generally noticed that the tentacles before becoming inflected, moved downwards and backwards, and that the upward and inflected movement subsequently took place. When a tentacle has become well inflected it can be observed that at the bending point the cells of the convex side are very turgid, with their plastoids spindle shaped, and that little or no aggregation has taken place, while in the cells of the concave side, on the other hand, there is well-marked aggregation and loss of turgidity ; the aggregation after a time extends to the convex side, the cells of which, in their turn, lose their turgidity, and at this stage all the cells are flaccid. Later on the cells of the concave side first regain their turgidity, and now the tentacle is bent back to its original position before stimulation. From certain observations on Dionæa and Mimosa, the author is led to believe that there also movement is made possible by the establishing of sudden and different conditions of turgidity of different cells, such differences being occasioned by the induced porosity of the protoplasm of certain of these cells. These phenomena occur perhaps in all cases of movement.

The plastoid markedly decreases in size after long stimulation in both Dionæa and Drosera. There are therefore some grounds for believing that it consists mainly of some reserve material or some substance which is used up during secretion. Whether the crystalloids in the nucleus of Pinguicula serve a similar purpose remains to

be seen, for at any rate a plastoid is not present. A somewhat casual examination was also made of many organs of movement, but in them no plastoid was observed. In *Drosera rotundifolia* and other species plastoids occur which resemble those of *Drosera dichotoma*. Strong single induction shocks or tetanising currents cause the plastoid to assume the spherical condition, or very frequently to break up into a string of small spheres. A sudden blow on the cover slip also causes the assumption of the spherical form. Moderately strong tetanising shocks cause swelling of the protoplasm, and increase of rapidity of movement and granularity. Very strong shocks may cause the contraction of the primordial utricle from the cell-wall at certain small areas, but immediate death always ensues, since the stimulus required is abnormally great. The normal effect of a regulated stimulus is to induce a swelling of the protoplasm and a loss of turgidity, and in consequence of the unequal reaction of the various cells to such a stimulus, movement of the tentacle also occurs.

[NOTE.—I have decided to name the body which I have provisionally spoken of as the plastoid "the rabdoid" (Gk. *rabdos*, a stick or wand). The change in form of the rabdoid appears to be a consequence of the molecular changes in the protoplasm. Differences of turgidity are among the results of these changes.—Nov. 28, 1885.]

VI. "On Variations in the Amount and Distribution of Fat in the Liver-Cells of the Frog." By J. N. LANGLEY, M.A., F.R.S., Lecturer on Histology in the University of Cambridge. Received September 23, 1885.

I have in a previous paper* mentioned some of the changes which occur in certain circumstances in the number and arrangement of the fat-globules in the liver-cells of the frog. From observations made since that time at different seasons of the year, I have been able to ascertain certain points undetermined in the previous account.

Variations in the Amount and Distribution of Fat with the time of Year.—The fat in the liver-cells is at its maximum amount in February and March. In January it is, as a rule, somewhat less. In April it rapidly decreases; from May until December it is present in comparatively small though varying amount. It is usually present in minimum amount in September and October.

Generally speaking, the fat-globules form an inner zone in frogs which have hungered more than a week. In January, February, and March, however, the fat-globules are commonly more numerous in the outer part of the cells, often forming a distinct outer zone.

* "Proc. Roy. Soc.," vol. 34, p. 20.

Sometimes the globules stretch throughout the cells. In December, the fat-globules may be more numerous in the outer part of the cells, but more commonly they are absent from the outer cell-region and form an inner zone. At other times of the year also, the fat-globules may be present in the outer portion of the cells, but this is comparatively rare.

From April to November, including those months, the globules may be very few and small. In December this is, so far as I have observed, very rare. In January, February, and March, I have always found fat-globules to be present in considerable number.

Effect of Temperature.—In December, when the fat in the liver is increasing in amount, cold increases the amount of fat stored up, and warmth decreases it.

The increase of fat, consequent on a decrease of temperature, occurs chiefly in the outer part of the cells. The fat-globules which are formed are fairly large.

The decrease of fat, consequent on increase of temperature, occurs chiefly or wholly at the outer part of the cells; as a rule, the number of globules in the inner part of the cells is increased. The decrease in the amount of fat, whilst due in part to a decrease in the number of fat-globules, is due to a still greater degree to their decrease in size.

Although warmth lessens the amount of fat in the liver in winter-frogs, it does not cause the fat to disappear entirely. The effect varies; in some frogs which have been kept seven to ten days at 22° C., the liver may still have considerably more fat than is ordinarily present in the liver in summer-frogs.

In summer-frogs, in which the fat-globules form an inner zone, and are nearly or entirely absent from the outer cell-region, neither warmth nor cold affects to any great degree the number or position of the fat-globules. They usually diminish somewhat in number and size with increase of temperature, but this is not necessarily the case. Variations of temperature have then much greater effect on the amount of fat in the liver in winter than in summer, *i.e.*, whilst in winter the ratio of fat formed to fat metabolised is greater in the cold than in the warm, in summer this is not necessarily the case.

Although summer-frogs have sometimes very little fat in the liver, it is doubtful whether the smallness of the amount is due either to the warmth of summer or to hunger, for we have seen that warmth does not necessarily cause a disappearance of fat from the liver; and frogs after long hunger have not infrequently a fair amount of liver-fat left. Hence the ratio of fat formed to fat metabolised, depends in part upon certain unknown conditions of the body, independent of temperature or of food.

Effect of Digestion.—When frogs are fed, *e.g.*, with worms, the fat in the liver at first decreases, after some hours it begins to increase and becomes greater than at the beginning of digestion; towards the end of digestion it decreases again in amount, so that in one or two days the amount is normal. Whilst the fat is decreasing in amount, the globules usually decrease in size; whilst the fat is increasing in amount, the globules usually increase in size, and are found in the outer region of the cells. Later, as the fat returns to normal, the globules form more and more an inner zone.

The extent of the changes just mentioned as occurring during digestion, as well as the period of digestion at which they occur, varies very considerably at different times of the year, and in different frogs. Winter-frogs which have before digestion much fat in the liver, show a decrease and subsequent increase of fat during digestion, but the increase is commonly not more than sufficient to bring the fat up to the amount present before food was given.

In some cases in summer-frogs, especially in September and October, the effect of feeding is very slight. There are also certain differences in the distribution of the fat-globules in different frogs, in the several stages of digestion. Usually when there are many fat-globules present in the outer part of the cells before digestion, they increase in number during the first stage of digestion in the *inner* part of the cells; but this is not always the case, whilst disappearing from all parts of the cell, they may disappear much more rapidly from the inner than from the outer cell-region.

Further, when fat-globules are present in the inner zone only, it may happen that, although they increase somewhat in number in the cells in the later stages of digestion, they do not accumulate in the outer cell-region, but are sparsely scattered in the outer cell-region, and are more numerous in the inner cell-region.

Probably these differences are to be accounted for in the following way.

We have seen that when fat is present in considerable amount, it is present almost without exception in greater quantity in the outer than in the inner part of the cells, and that in most cases, as it is increasing in amount, whether from decrease of temperature, or from digestion, it increases in the outer part of the cells, frequently being found close to the basement membrane. Hence we may conclude, that fat is formed more rapidly in the outer than in the inner part of the liver-cells.

We have seen that when winter frogs are kept in the warm, it is in the outer portion of the cells that the fat chiefly disappears, and that in summer-frogs, fat is rarely found except in the inner cell-region. Hence probably the metabolism as well as the formation of fat is more rapid in the outer than in the inner cell-region.

Further we have seen that in winter-frogs kept in the warm, and in fed winter-frogs, there is commonly an increase in the number of fat-globules in the inner zone although the total amount of fat is much diminished. Hence probably there is in certain circumstances a transference of fat-globules from the outer to the inner part of the cells. The amount of fat present in the liver of course depends upon the relative rates of formation and metabolism of fat, and as this varies in different circumstances, so probably the rate of transference of fat from the outer to the inner cell-region varies in different circumstances. To this the differences spoken of above are probably due. In summer frogs for example, whilst in the later stages of digestion fat is formed more rapidly in the outer cell-region, an accumulation of fat in this region may not take place, partly on account of the more rapid metabolism of the fat formed, and partly on account of its more rapid transference to the inner cell-region.

Some of the fat-globules in the inner zone are no doubt passed out of the cell with the bile secreted, for some small fat-globules are always present in the bile.

One other conclusion we may draw with regard to the fat-globules: since they diminish in size under the influence of warmth, and in the first stage of digestion, it is probable that each separate fat-globule is slowly metabolised in the same way that mesostate granules in secretory glands are metabolised. The granules of the salivary glands, of the gastric glands, of the pancreas, are not dissolved as a whole during secretion, they are dissolved steadily and gradually.

The Effect of Peptone and of Dextrin.—From June to August, peptone or dextrin, when injected into the dorsal lymph-sac of a frog, produces changes like those produced by feeding; *i.e.*, there is at first a decrease in the amount of fat in the liver, then an increase chiefly in the outer part of the cells, and this is succeeded by a decrease to the normal state, during which the fat-globules become shifted from the outer to the inner cell-region. The increase of fat is usually greater with dextrin than with peptone. As a rule the increase in the amount of fat takes place three to four hours after peptone has been injected; in twenty to thirty hours after the injection, the fat-globules are present, as at starting, in the inner cell-region only. The times at which the various changes take place vary, however, in different frogs. If frogs are kept in the warm and peptone is injected, the effect on the fat in the liver is much less than when the frogs are at the ordinary temperatures. In September the effect of peptone appears to be less than from June to August. In the winter months I have not made a sufficient number of experiments to be certain what changes peptone and dextrin then produce.

Sewall has pointed out that peptone injected into the dorsal sac of a frog, causes, in one to two hours, the stomach to be distended with

fluid of a neutral or slightly acid reaction. This distension of the stomach occurs in most but not in all cases. The fluid is, I find, usually alkaline, sometimes both the mucous fluid in the stomach and the stomach-wall itself are strongly alkaline. The fluid contains a small amount of pepsin. Dextrin produces a similar but less distension of the stomach, the mucous fluid in this case is usually acid. I may mention that digestion in the frog is delayed very greatly by peptone, and slightly by dextrin. The stimulus set up by the fluid in the stomach may have something to do with the changes in the liver which follow injection of peptone or of dextrin. But since the changes in the liver occur in the cases in which the injection causes little or no formation of fluid in the stomach, I attribute them in the main to the direct action of the substance injected. This I am the more inclined to do since I find that both peptone and dextrin cause in a few hours an accumulation of glycogen* in the liver in summer-frogs.

November 26, 1885.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election, was read as follows:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.—Professor Robert B. Clifton, M.A., Professor James Dewar, M.A.; Professor William Henry Flower, LL.D.; Archibald Geikie, LL.D.; Sir Joseph D. Hooker, K.C.S.I.; Professor Thomas Henry Huxley, D.C.L., LL.D.; Admiral Sir A. Cooper Key, G.C.B.; J. Norman Lockyer, F.R.A.S.; Professor Henry N. Moseley, M.A.; Professor Bartholomew Price, M.A.; Reverend Professor Pritchard, D.D., F.R.A.S.; William James Russell, Ph.D.; Professor J. S. Burdon Sanderson, LL.D.; Professor

* Seegen has shown that peptone increases the amount of sugar in the liver.

Arthur Schuster, Ph.D.; Lieutenant-General R. Strachey, R.E., C.S.I.; General James Thomas Walker, C.B.

Surgeon-Major James Edward Tierney Aitchison (elected 1883) was admitted into the Society.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On the Fertilised Ovum and Formation of the Layers of the South African Peripatus." By ADAM SEDGWICK, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. Received October 18, 1885.

The Fertilised Ovum and its Nucleus.

The fertilised ovum of *Peripatus capensis* contains a central cavity traversed by a few strands of protoplasm.

The protoplasm has a reticular or spongy structure, the reticulum being very close round the nucleus and much looser elsewhere. The strands of the reticulum are composed of an apparently perfectly homogeneous protoplasm. The nucleus is placed on one side of the ovum; it is a large body which varies very considerably in shape and structure in different ova. These variations no doubt represent different phases in the life-history of the nucleus. It has been impossible for me with the small number (ten) of unsegmented ova at my disposal to determine their sequence.

The nucleus of the fertilised ovum is, however, so large and favourable for study that I have thought it worth while to describe three different stages in which I have seen it.

1. A spherical structure (diameter, 0·04 mm.) bounded by a membrane, which is slightly indented at one point, where it sends in a prolongation of itself, which passes through the nucleus to become continuous with the membrane of the opposite side. The nucleus is made up of a fine spongework of very pale fibrils, *which are continuous with the nuclear membrane and with the septum* just mentioned. In this spongework are a number of deeply-staining more or less spherical bodies. The nuclear membrane and septum appear precisely similar in structure to the strands of the external protoplasmic reticulum, and *the latter are continued directly into* the former. The pale nuclear reticulum is also similar to the extra-nuclear reticulum, differing only

in intensity of staining. The deeply-staining bodies in the nucleus are, I think, placed in the strands of the pale reticulum; but of this I am not certain.

This nucleus then consists of a simple spongework of protoplasm, differing only from the external protoplasm in the fact that the staining matter is aggregated into small masses and into the septum already mentioned. The apparent nuclear membrane is simply part of the protoplasm at the junction of the modified (nuclear) and unmodified (cell-substance) part of the protoplasmic network.

2. A form closely resembling the above, except in the fact that the nuclear spongework is stained slightly, though not quite so deeply as some of the extra-nuclear protoplasm. There are only two small deeply-staining masses, which are not so conspicuous as in the first form.

We may state the difference between these two nuclei thus: in the first form the chromatin of the nucleus is aggregated into a number of small masses, while in the second form the chromatin is diffused throughout the nuclear reticulum. The word chromatin being used to denote the property which enables the protoplasm to take up and retain the staining matter. The extra-nuclear protoplasmic threads possess this property, and may be said to possess chromatin, but it is in a diffused form, as in the second form of nucleus.

3. In the third form the nucleus is divided by a number of septa, radiating from its centre, into chambers. The chambers are partially divided up into secondary chambers by prolongations of the septa. The septa are continuous externally with the extra-nuclear protoplasmic reticulum. It is impossible to speak of a distinct boundary of the nucleus in this form, and the substance of the nuclear septa and their prolongations is exactly similar in appearance and staining properties to the strands of the surrounding protoplasmic network or spongework.

A number of chromatin masses occur in each chamber of this radiate nucleus—they appear to lie in the offshoots of the septa into the chambers and in delicate expansions of these. But it is impossible to determine exactly the relation of these chromatin globules to the protoplasmic network in the nucleus.

This form of nucleus is most interesting, because were it not for the chromatin masses the nucleus would be quite undistinguishable from the surrounding protoplasm, except, perhaps, by the fact that the meshes of the network (*i.e.*, network as seen in section) are rather larger than in the protoplasm immediately around the nucleus.

The most important, and at the same time most certain, of these observations on the nucleus of the fertilised ovum of Peripatus, is that the intra-nuclear and extra-nuclear reticulum are both continuous with the so-called nuclear membrane.

4. The last form I have to describe is the spindle form. It was met with in an ovum of two segments.

The spindle is of enormous size (distance between the poles 0·06 mm.). The protoplasmic fibres composing it are absolutely the same in appearance as the rest of the cell protoplasm. The chromatin is present in a very condensed form (*i.e.*, deeply staining) as a number of bent rods at the equator of the spindle. Around the poles of the spindle the protoplasmic reticulum is arranged in a radiate fashion. The spindle appears not to be composed of simple fibres running from pole to pole, but of the ordinary reticulum, the meshes of which are very much elongated in a direction parallel to the long axis of the spindle.

The facts which are most clearly brought out by the above observations, and about which I have no doubt, are—

1. The continuity of the nuclear reticulum with the extra-nuclear reticulum in form 3 (and almost certainly in forms 1 and 2).

2. The similarity in structure and continuity between the so-called fibres of the spindle in form 4 with the surrounding reticulum; and the conclusion I have drawn from my observations is that the nucleus of the fertilised ovum of Peripatus differs from the cell protoplasm only in the manner in which the so-called chromatin contained in the protoplasmic meshwork (both of nucleus and rest of ovum) behaves. In the nucleus it varies from a state of diffusion through the reticulum to a state in which it is condensed into the chromatin masses of form 1.

In the subsequent stages of segmentation, the nucleus gradually becomes smaller until at the close of segmentation it has an oval form with a long diameter of 0·016 mm. It now presents the features described by Flemming and other observers in the nuclei of the salamander. I have seen all through the development of Peripatus after segmentation most of the stages figured from the salamander.

During segmentation the nucleus generally has the third form above described: I have never seen it in a spherical, and only once in a spindle form. I conclude that these forms if they occur are very rapidly passed through.

The Segmentation.

I have already described this in a general manner in my communication of May last, and with figures in the "Quarterly Journal of Microscopical Science" of July last.

I then stated that the endoderm cells were connected with each other by processes ("Quart. Journ. Micros. Sci.," xxv, Plate 31, fig. 8). I have now the following facts to add:—

1. The so-called endoderm cells are at first without a distinct nucleus, they do not get a nucleus until just before the gastrula stage.

2. All the cells of the ovum, ectodermal as well as endodermal, are connected together by a fine protoplasmic reticulum which is placed, as are also the cells, immediately beneath the egg membrane, and therefore around a central space.

Each ectoderm cell consists of a central nucleus around which is a close protoplasmic spongework, which at the outer parts of the so-called cell becomes of a gradually looser nature until it runs into the spongework of the surrounding cells.

Each endoderm mass consists of a central denser spongework which gradually becomes looser towards the periphery of the mass until it is continued into a fine reticulum. The endoderm masses are far apart from each other and are connected by this reticulum.

The continuity of the various cells of the segmenting ovum is primary and not secondary, *i.e.*, in the cleavage, the segments do not completely separate from one another. But are we justified in speaking of cells at all in this case? *The fully segmented ovum is a syncytium, and there are not and have not been at any stage cell limits.* I think the cleavage should be rather described not as segmentation, but a multiplication of the nucleus or centre of force which causes a corresponding readjustment in the density of the network at different parts of the ovum, but no break in continuity.

The Structure of the Gastrula.

The formation of the gastrula, which is at first solid, has been described in my first communication to the Society on this subject.

The endoderm masses at first, as I have already mentioned, have no nuclei. Nuclei first appear in them during the progress of the epibole by which the gastrula is formed. I have not been able to determine the origin of these nuclei. They either arise *de novo* in the endoderm masses or migrate into the latter from the ectoderm. The protoplasmic network at the centre of each endoderm mass is denser than at the periphery, but is without the chromatin granules, so characteristic of a nucleus. But I have already described a stage of the nucleus in the fertilised unsegmented ovum in which the chromatin granules are almost entirely absent, and in which the network presents no essential difference from the surrounding network. Again, another in which the nuclear network merges so gradually into the surrounding network, that it is impossible to point to any limit between them. I therefore think it quite possible that this central denser protoplasm in the endoderm masses may give rise to the nucleus which subsequently appears. But on the other hand I must distinctly state that this is only a possible view which may be borne in mind, but which cannot be accepted without the most overwhelming proof in the present state of our science.

The solid gastrula is a syncytium; the ectodermal nuclei are

arranged round the periphery of the ovum, while the endodermal nuclei are within. The latter are characterised by their angular shape, and by never presenting the karyokinetic figures characteristic of the ectodermal nuclei. The protoplasm of this syncytium is much vacuolated throughout, but the vacuoles are largest in the centre. These central vacuoles unite and give rise to the gut cavity, which opens to the exterior through a point on the surface where the ectodermal nuclei have always been absent. This opening is the blastopore. The blastopore, until quite late in development, is traversed by protoplasmic strands, which anastomose with similar strands projecting from the protoplasm lining the large central vacuole or gut.

The gut of Peripatus arises, therefore, as *a vacuole in a multinucleated mass of protoplasm*, and the gastrula of Peripatus is a multinucleated mass or syncytium, with absolute continuity of the protoplasm of all parts of the ovum.

The Origin of the Mesoderm.

After the definite formation of the blastopore, an area of protoplasm, placed in the ectodermal layer of the syncytium, and characterised by possessing several nuclei less densely packed together than elsewhere, is distinctly visible in the middle line of the ventral surface just behind the blastopore. I cannot be certain of the exact number of the nuclei belonging to this area in the youngest embryo in which I observed it, as the limits of the area are difficult to determine by inspection of transverse sections. However that may be, the area has in transverse section very much the appearance of the pole cells of other forms, and is the structure described in my first paper as "some cells which cannot be definitely assigned to the ectoderm or to the endoderm, at the hind end of the blastopore." Its nuclei undergo division and give rise to the densely packed mass of nuclei of the primitive streak.* A part of it seems to persist for some time in the deeper parts of the primitive streak close to the endoderm: I have not yet succeeded in tracing the fate of this portion.

The nuclei of the primitive streak migrate forwards between the ectodermal and endodermal nuclei, and take up their position in the protoplasm intervening between the latter.

These rows of nuclei are the mesodermal bands. They soon arrange themselves into groups around a central vacuole, and so give rise to the most conspicuous parts of the mesoblastic somites. I leave the ovum for the present at the commencement of the formation of the somites, merely stating that it is still a syncytium.

* It is possible that some of the ectoderm nuclei adjoining this area may take part in the production of the primitive streak nuclei.

The facts above recorded are somewhat novel. That they are facts so far as *Peripatus capensis* is concerned I have not the slightest doubt. Whether or no they are applicable to other animals is another question. If they are, the following considerations present themselves:—

1. Klein's view of the continuity between the reticulum of the nucleus and the reticulum of the extra-nuclear protoplasm receives striking confirmation.

2. Metschnikoff's and Lankester's views as to the origin of the gastrula and its gut receives support.

3. Herbert Spencer's view of the origin of the nervous system may perhaps not be so far from the mark as at first sight appeared.

4. The connexion between the nerve and muscles and sensory epithelial cells receives its morphological explanation, being due to a primitive continuity which has never been broken. In fact the connexion between almost every kind of tissue cell is explicable as being the primitive condition.

5. There is no essential difference between ducts with perforated cells and ducts with so-called cellular walls (inter- and intra-cellular ducts).

6. If the protoplasm of the body is really a syncytium, and the ovum until maturity in the ovary a part of that syncytium, the separation of the generative products does not differ essentially from the internal gemmation of a protozoon, and the inheritance by the offspring of peculiarities first appearing in the parent, though not explained, is rendered less mysterious, for the protoplasm of the whole body being continuous, changes in the molecular constitution of any part of it would naturally be expected to spread, in time, through the whole mass.

Shortly, these facts if generally applicable reduce the adult body to a syncytium—to a multi-nucleated vacuolated protoplasmic mass, and embryonic development to a multiplication of nuclei and a specialization of tracts in this mass.

II. "On the Formation of the Mesoblast, and the Persistence of the Blastopore in the Lamprey." By ARTHUR E. SHIPLEY, B.A. Communicated by Professor M. FOSTER, Sec. R.S. Received October 29, 1885.

At the close of segmentation the egg of the Lamprey (*Petromyzon planeri*) forms a blastosphere. Owing to the way in which the yolk is distributed, the segmentation cavity is rather eccentrically placed. It is roofed in by several rows of small cells, while its floor is composed of a few very large cells much crowded with food yolk. The

small cells pass gradually into the larger ones at the sides of the segmentation cavity. All the cells of the blastosphere are crowded with yolk spherules, which are, however, very much smaller in the upper cells, where active division is going on, than in the more inert lower cells. The latter may be conveniently termed the yolk cells. The segmentation cavity is considerably larger than that of the frog's ovum at a similar period. The next stage in the development of the ovum is accompanied by the thinning out of the upper layer of cells, until the roof of the segmentation cavity finally consists of a single layer of cells. On this point my observations confirm those of Calberla, and are opposed to those of Max Schultze, who found a many-layered roof covering the segmentation cavity just before invagination. The layer is composed of epiblastic cells, and in this respect resembles the many-layered roof of the segmentation cavity in the frog's ovum of the same stage. The lower cells of the many-layered roof seem to pass round to the sides and floor of the segmentation cavity, so that about the fiftieth hour after fertilisation the upper half of the egg consists of a hemispherical segmentation cavity, roofed in by a single layer of cells, the lower half being solid and composed of yolk cells. Viewed as an opaque object, the upper half is of a whiter colour than the lower.

The invagination to form the mesenteron takes place in the region where the single layer of epiblast cells passes into the yolk cells. In my eggs the first trace of this invagination appeared about 130 hours after artificial fertilisation. The mouth of the invagination or blastopore is at first a wide arched slit, which subsequently narrows to a round hole. From the first sign of invagination, a cavity, the mesenteron, is present; in this the Lamprey resembles Amphioxus, but differs from the Frog, where the mesenteron is formed as a slit some time after the invagination has begun. It differs from Amphioxus, however, in the fact that the invagination is not symmetrical, being like the segmentation cavity, pushed dorsally by the accumulation of yolk cells at the lower pole. The upper layer of invaginated cells retain the character of the epiblast cells, the lower are larger, and have the characters of the yolk cells. The former lie close against the inner surface of the epiblast cells in the dorsal median line, here again differing from the Frog, where a mass of cells, which subsequently form mesoderm, lies dorsally to the invaginated cells and between them and epidermis. There is thus no mesoblast present at the dorsal rim of the blastopore, such as is found in frog's egg. During these processes the epiblastic cells have gradually enclosed the yolk cells; this appears to take place by the conversion of the outer yolk cells into epiblast cells, and takes place latest in the region of the blastopore. The mesenteron continues to deepen, and as it increases in size the segmentation cavity diminishes, and is finally obliterated. The roof and

sides of the mesenteron consist of columnar cells, in appearance very like the epiblast, against the inner surface of which it is closely pressed. The floor is composed of cells, which retain their yolk-like characteristics for a considerable time. The mesoblast now begins to appear by the differentiation of two bands of those yolk cells which lie in the angles formed by the invaginated mesenteron and the epiblast. The differentiation appears to take place from before backwards. The two bands of mesoblast are separated from one another in the dorsal median line by the juxtaposition of the invaginated hypoblast and the epiblast. They are separated ventrally by the hypoblastic yolk cells which are in contact with the epiblast over the lower two-thirds of the egg. Subsequently, but at a very much later stage, the mesoblast is completed ventrally by the downgrowth on each side of the mesoblastic plates. These proliferate cells at their edge, which grow down between the hypoblastic yolk cells, and so complete the mesoblast ventrally. The first formation of the longitudinal band appears to take place by a differentiation of hypoblastic cells *in situ*, and not by an invagination of cells.

This account of the origin of the mesoblast differs materially from that given by Scott. According to his observations, the mesoblast is derived from two sources: (1) cells which are invaginated with the mesenteron, these form the longitudinal bands; (2) the outer layer of hypoblastic yolk cells, which split off the remainder, and form the ventral sheet which completes the mesoblast on that side of the body. Since by this time the head of the embryo has raised itself above the yolk, there are no hypoblastic yolk cells in it, and consequently its mesoderm is entirely derived from the first source, whereas in the trunk the dorsal mesoderm is derived from the first source, the ventral from the second.

By the time that the mesoblastic plates become separated from the yolk cells, the neural plate becomes evident in the exterior. This extends from the blastopore as a low ridge, over two-thirds of the circumference of the egg. Its appearance is soon followed by the separation of the anterior end from the rest of the yolk.

The invaginated endoderm has extended round for more than half the circumference of the egg, and its most anterior portion is included in the head, which is by this time distinct from the rest of the embryo.

All this time the blastopore has been visible at the posterior end of the neural plate. It has been figured in this position by Schultze, who gives a very complete set of figures of the embryo viewed as a whole. The elongation of the embryo now proceeds so rapidly that the anterior end curves round over the blastopore; the posterior end is much the largest, containing all the food yolk.

Schultze, from observations upon the whole embryo, came to the

conclusion that blastopore persisted as the anus, and this view was supported by Calberla. On the other hand the later observers, who have studied the development by means of sections, have maintained Benecke's view that the blastopore closes. Scott describes the neural canal as enclosing the blastopore, and by its closure forming a neurenteric canal, which he figures. He states that the anus is subsequently formed by a protrusion of the alimentary canal against the skin, which becomes open about the twentieth day. Balfour states that the blastopore closes, and does not form the permanent anus.

My observations on the embryo as an opaque object led me to the belief that the blastopore remained open, and in this I have been confirmed by a number of series of sections taken from embryos of all stages, from the commencement of the invagination until the time when the cloaca is definitely established. At its first appearance the blastopore is situated at the posterior dorsal surface of the embryo, but by the elongation of the embryo and the formation of the tail the blastopore comes to occupy a position on the ventral surface.

Scott was of opinion that the lumen of the invaginated mesenteron persisted only in the fore-gut. This part of the alimentary canal shortly after the invagination is completed is raised with the head from the rest of the embryo. This part is therefore free from the large yolk cells, and the cells lining the mesenteron soon assume a definite columnar character, although they continue to contain yolk granules for a considerable time. According to Scott and Calberla the lumen of the mesenteron in the trunk entirely disappears, and only appears again at a much later stage. My sections, however, show that the lumen never really disappears. At its anterior end, as is just mentioned, its lining cells soon become columnar, and these extend from its blind anterior end to the posterior part of what will subsequently form the gill region. A similar change takes place at the posterior end. The cells surrounding the blastopore, and extending for some distance into the alimentary canal, very early assume a columnar character, and are in fact indistinguishable from the epidermal cells. The cells lining the mid-gut do not assume this epithelial-like character till a much later stage. The dorsal row are, however, more columnar than those on the ventral side; these latter have just the same characters as the other yolk cells.

At its posterior end the neural tube becomes solid, and this solid rod soon fuses with the posterior end of the notochord, and with a solid rod of cells which pass backwards from the hind-gut, and probably represent the post-anal gut. A little further back the mesoblastic plates join this mass of indifferent tissue; so that we have behind the anus, in a position corresponding to the front lip of the blastopore, when it occupied its primitive position on the posterior dorsal surface,

a mass of indifferent tissue, into which pass representatives of all three germinal layers. This must represent the primitive streak.

The persistence of the blastopore to form the anus has been demonstrated in the Amphibia, by Miss Johnson in the Newt, by Gasser in Alytes, and by Spencer in the Frog. The fact that it persists in the Cyclostomata appears to point to the fact that this is a primitive feature retained in those eggs which have not become much modified by the presence of a large mass of yolk. This view would be greatly confirmed if renewed observation on the development of Amphioxus should demonstrate the same fact.

III. "Researches on Myohæmatin and the Histohæmatins." By C. A. MacMunn, M.A., M.D. Communicated by Professor M. Foster, Sec. R.S. Received October 19, 1885.

(Abstract.)

This paper contains an account of observations made on the spectra of the organs and tissues of invertebrates and vertebrates, which have brought to light the presence of a series of animal colouring matters which had not previously been discovered.

The name histohæmatins is proposed for all these colouring matters, and that of myohæmatin for the intrinsic pigment occurring in striped muscle, which belongs to the same series.

These pigments are not identical with any known decomposition product of hæmoglobin, and they are found in animals in whose bodies no hæmoglobin can be found.

The method of examination is as follows:—The tissue or part of organ to be examined is put into a compressorium, by means of which any required thickness can be obtained, it is illuminated by means of a large sub-stage condenser, and examined with a Sorby's microspectroscope fitted to a binocular microscope, the binocular form being preferred, as one tube is free for the observation of the specimen. The source of illumination was generally an argand gas burner, sometimes direct sunlight, sometimes a Swan lamp. The objectives of the microscope up to the one-eighth were so adapted as to enable both fields of the microscope to be fully illuminated, which is a matter of importance in dealing with small quantities of material, or in differentiating those portions of an object which give different spectra.

The Histohæmatins.—Examined in this way the organs and tissues of invertebrates and vertebrates present a series of spectra, which are all evidently connected with each other. From Echinoderms to man the same appearances have been found; thus there is a most striking

likeness between the spectra of such organs as the ovaries of a starfish and the pancreas, stomach-wall, kidney, and other organs of a cat, and between these and that of striped muscle throughout the whole animal kingdom.

To give an idea of the character of these spectra and their distribution is not easy in an abstract, but in general they may be said to consist of at least three bands—one before D, one or two between D and E, and sometimes one or two others nearer violet. When one band occurs between D and E it replaces the two found in other cases, which are of great narrowness compared with other physiological spectra. The band before D is always the same both in the histohæmatins, and in myohæmatin with few exceptions. Sometimes this kind of spectrum is replaced by another in which two narrow bands like those of reduced hæmatin occur nearer the violet than the bands of the latter. It was proved repeatedly that the *banded* spectrum belongs to the deoxidised condition, and the *bandless* to the fully oxidised; accordingly these pigments are respiratory.

Among animals in which I have found these spectra a few may be enumerated with the organs in which they occur. Thus in Echinoderms the *ovaries*, *stomach-wall*, and other parts of *Uraster rubens* show them well marked.

In molluscs, *Limax flavus*, *L. variegatus*, *Arion ater*, *Helix aspersa*, *H. pomatia*, and other slugs and snails contain them in such parts as the *nephridium*, *albumen gland*, *ovo-testis*, *receptaculum seminis*, *foot*, *wall of crop*, *oviduct*, *penis* and *elsewhere*, and all contain myohæmatin in the muscle of the heart in both auricle and ventricle, also in the pharyngeal muscle. In these species, as Dr. Sorby first showed, the bile contains a kind of hæmatin, which is evidently connected with the histohæmatins for reasons given in the complete paper. In other molluscs such as *Littorina littorea*, *Purpura lapillus*, *Trochus cinerarius* and *ziziphinus*, *Patella vulgata*, *Limnæus stagnalis*, *Mytilus edulis*, *Ostraea edulis*, *Anodonta cygnea*, and others, they have been observed by the above method of examination.

In *Arthropods*, such as *Homarus vulgaris*, *Astacus fluviatilis*, *Cancer pagurus*, *Carcinus moenas*, and *Pagurus Bernhardus*, they have also been found, in such situations as the *green glands* (of the two first), the *stomach-wall*, *liver*, exceptionally in the *branchiæ*, and elsewhere. They are also present in insects and in spiders.

In all these invertebrates (and others) they can be studied uninfluenced by the presence of hæmoglobin. Wherever they are seen the corresponding tissue or organ is more or less yellow or reddish-yellow, but sometimes almost colourless.

On examining vertebrates I was surprised to find the same spectra in such situations as the *liver*, *spleen*, *kidney*, *stomach-wall*, *pancreas*, *wall of intestine*, and sometimes in the *ovary*. And by washing out

the blood-vessels with salt solution, I found the bands became much better marked.

Thus:—In fishes they have been observed in the tench, herring, roach, eel, and others.

In reptiles, in *Tropidonotus natrix*, *Bascanium constrictor*, *Scincus officinalis*, *Trionyx*, *Emys Europæa*, *Lacerta viridis*, and *Lacerta agilis*.

In Amphibians, in *Rana temporaria*, *Hyla arborea*, *Bufo vulgaris*, *Salamandra maculosa*, *Siredon pisciformis*, and others.

In *birds*, *e.g.*, *pigeon*, *owl*, *turkey*, *goose*, *duck*, *swift*, &c. The *gizzard* of birds is, however, mainly coloured by oxyhæmoglobin.

In *Mammals*, *e.g.*, *dog*, *cat*, *rat*, *rabbit*, *guinea-pig*, *hedgehog*, *sheep*, *ox*, *pig*, *mouse*, and *man*, as well as in others.

In all cases oxidation and reduction could be brought about. When the bands are invisible they can be brought into view by dipping the portion of organ or tissue into Stokes's fluid, or into a weak solution of ammonium sulphide in water; on exposure to the air they become faint.

Myohæmatin.—All the species enumerated and others have been examined for *myohæmatin*, and in all those which possess striped muscle it has been found.

Thus in molluscs, it is found in the heart and pharyngeal muscle of *Limax*, *Arion*, *Helix*, and other pulmonates.

In *Arthropods*, in the cardiac muscle of *Homarus*, *Astacus*, *Cancer*, *Carcinus*, and *Pagurus*, and not in their voluntary muscles (so far); also in the muscle of the cephalo-thorax of such spiders as *Epeira diadema*, *Tegenaria civilis* (and others).

In *insects* it is abundantly present, especially in the muscles from the thorax. It is best marked in those which move the wings actively, such as diurnal and nocturnal lepidopters. So far it has been found in the following insects:—*Musca vomitoria*, *domestica*, and *chlora*, *Apis mellifica*, *Bombus terrestris*, *Vespa vulgaris*, *Hydrophilus*, *Dyticus marginalis*, *Geotrupes stercorarius*, *Lucanus cervus*, *Coccinella*, *Staphylinus olens*, *Cerambyx moschatus*, *Creophilus maxillosus*, *Carabus violaceus*, *Periplaneta orientalis*, *Gryllus domesticus*, *Acrida viridissima*, *Tipula oleracea*, *Pieris rapae*, and various other lepidopters and dipters. It has also been found in the mouth parts of larvæ.

Among vertebrates it has been found in the heart and voluntary muscle of all the fishes, reptiles, amphibians, birds, and mammals enumerated above. It is frequently accompanied by hæmoglobin in these classes, and sometimes replaced by it. It may be apparently absent, but its bands may be brought into view by the use of reducing agents.

The spectrum of myohæmatin is remarkable for the sharpness and narrowness of its bands, in which point it and the other histohæmatins differ from any other known animal pigment. Three bands are

always present, one between C and D, close to D, which corresponds to the first band of the histohæmatin spectrum, two very narrow and sharp, of which the second is darker than the first between D and E; besides these there are one or two nearer violet, one covering E and *b*, the other before F, which are not always present. The 2nd and 3rd bands correspond closely to similar bands in the histohæmatin spectrum, in which they may appear joined to form one broad band.

The colour of the pigment giving this spectrum is yellow or reddish-yellow, but even in very pale muscles the bands can sometimes be seen, especially after the use of a reducing agent.

Various attempts have been made to isolate myohæmatin, but owing to the coloured constituent being joined to a proteid, it will not go into any of the usual solvents. It has been proved by pressing out the plasma from frozen muscle that some of the myohæmatin belongs to the plasma, but even after this treatment, its bands are seen better marked in the muscle than before. It has also been got from blood-free muscle by digesting in pepsin solution, which, however, alters the pigment, as it no longer gives the original spectrum, but another which is remarkable, as it is imitated closely by a spectrum sometimes seen in insect muscle without any treatment. Obtained by this method, the pigment is of a yellow colour, and is only soluble in water.

From the changes which the histohæmatins and myohæmatin undergo with oxidising and reducing agents, and for reasons given in detail in the paper, I have come to the conclusion that these pigments are concerned in the *internal* respiration of the tissues and organs in which they are found.

Spectrum of the Adrenals.—Another point of interest brought to light by these observations is the occurrence of hæmochromogen in the medulla of the supra-renal glands of mammals; thus in this situation in man, dog, cat, ox, sheep, pig, guinea-pig, rabbit, and rat, I have found hæmochromogen, the bands of which are very dark; and it would appear that this hæmochromogen is partially removed by washing out the blood-vessels with salt solution. Hence, and owing to the fact that elsewhere it is excretory, as in the bile, the hæmochromogen of the adrenals appears to be excretory; if so a downward metamorphosis of hæmoglobin, and probably (for reasons given in the paper) of the histohæmatins, is one of their functions. Hence if by disease, or by artificial removal, this metabolism is prevented, the incompletely metabolised pigments circulate in the blood, and staining of skin and mucous membrane, as in Addison's disease, may take place. In the urine of Addison's disease such an imperfect metabolite occurs as I have already shown.*

* "Journal of Physiology," vol. vi, p. 37.

To give an idea of the connexion which exists between the histohæmatins and myohæmatin, I have here added a few wave-length measurements. These are given in greater detail in the complete paper, and the spectra have been mapped to the number of 70 in the accompanying charts.

No. 1. A histohæmatin spectrum from the ovaries of a star-fish (*Uraster rubens*) :—

1st Band........	λ613—593
2nd „	λ569—560
3rd „	λ556—548·5

No. 2. A histohæmatin spectrum from the stomach-wall of a cat :—

1st Band........	λ613—593
2nd „	λ569—563
3rd „	λ556—551

No. 3. A similar spectrum from the pancreas of a cat :—

1st Band........	λ613—596·5
2nd „	λ569—563
3rd „	λ556—548·5

No. 4. A myohæmatin spectrum from *Hydrophilus* :—

1st Band........	λ613—593
2nd „	λ569—563
3rd „	λ557—548·5

No. 5. A myohæmatin spectrum from the heart of the lobster (*Homarus vulgaris*) :—

1st Band........	λ613—593
2nd „	λ569—563
3rd „	λ556—550

No. 6. A myohæmatin spectrum from the heart of a cat :—

1st Band........	λ613—596·5
2nd „	λ569—563
3rd „	λ556—550

It may be added that it is very difficult to measure these bands, and allowing for this fact the agreement is very close, especially as all the measurements were made independently of each other. It is also necessary to add that the above are what may be called typical histohæmatin spectra, as in some cases Bands 2 and 3 are joined to each other to form one band.

IV. "On the Geometrical Construction of the Cell of the Honey Bee." By HENRY HENNESSY, F.R.S., Professor of Applied Mathematics in the Royal College of Science, Dublin. Received October 20, 1885.

The well-known problem of the bee's cell occupied the attention of eminent mathematicians early in the last century, and it is still presented as an interesting example of geometrical maxima and minima. In 1743 Maclaurin communicated to the Royal Society a solution of the question, which appears in the "Philosophical Transactions,"* and it seems that the comparison between the mathematical results and the actual cells was effected by angular measurements. Long since a simple method occurred to me for the construction of the figure without employing angles, and as I have not been able to find it in any publication, I venture to submit it in this short paper.

A structure has a regular hexagon for its orthogonal cross section, and is terminated by three lozenges which meet in a trihedral angle; required the relation between the side of one of these lozenges and the side of the regular hexagon forming the cross section of the prism so as to give the smallest surface to the structure. The long diagonal of one of these lozenges is equal to the side of the equilateral triangle inscribed in the hexagon; if we call this E and the short diagonal e, then—

$$E=\sqrt{3}h \text{ and } e=\sqrt{h^2+4x^2},$$

where h is the side of the hexagon, and x the difference between the parallel sides of one of the six faces of the prism. The area of a lozenge is therefore $\frac{\sqrt{3}h\sqrt{h^2+4x^2}}{2}$, and that of the face of prism $\frac{h(2l-x)}{2}$.

For u, the total surface of the structure, we have

$$u=\frac{3h}{2}[\sqrt{3(h^2+4x^2)}+2(2l-x)],$$

and hence
$$\frac{du}{dx}=3h\left[\frac{2x\sqrt{3}}{\sqrt{h^2+4x^2}}-1\right].$$

This, equated to zero, gives $x=\frac{h}{2\sqrt{2}}$.

A side, s, of the lozenge is manifestly $\sqrt{h^2+x^2}$, hence

$$s=3x.$$

* Vol. 42.

A side of the lozenge is thus equal to three times the difference between the two parallel edges of the trapezium. If a line equal to the side of the hexagon be bisected by an equal line at right angles, and the extremities of both joined, half the resulting line will be equal to x, so that *the side of one of the lozenges is three times the half side of a square whose diagonal equals the side of the hexagon.* It is moreover manifest that

$$\frac{d^2u}{dx^2}=\frac{6\sqrt{3}h^3}{(h^2+4x^2)^{\frac{3}{2}}}=2\left(\frac{h\sqrt{3}}{\sqrt{h^2+4x^2}}\right)^3,$$

which is positive for $x=\frac{h}{2\sqrt{2}}$, and therefore the surface u is the least possible. As the angle at top of the trapezium has for its tangent $\frac{h}{x}=2\sqrt{2}$, this angle is 70° 31′ 44″, the supplement of 109° 28′ 16″ for the obtuse angle of the lozenge, found by Maclaurin.

It is manifest that $E=h\sqrt{3}$ and $e=\sqrt{2}E$, or the longer diagonal, is equal to the diagonal of a square whose side is the shorter diagonal of the lozenge. Also the acute angle of the lozenge is equal to the acute angle of the trapezium, for the tangent of half the former is $\frac{1}{\sqrt{2}}$.*

From these results a model of the bee's cell can be easily constructed, with the aid of a pair of compasses, as follows:—

1. Inscribe an equilateral triangle in a hexagon; a side of this triangle is the long diagonal of the lozenge; bisect this, and the diagonal of a square erected on the half is the shorter diagonal of the lozenge.

2. Draw six parallel lines at distances equal to the side of the hexagon, and a straight line perpendicular to them from the second of the parallel lines; inflect a straight line equal to a side of the lozenge above constructed, and repeat this process until six trapeziums are finished.

3. On folding these trapeziums a hexagonal prism is formed into which three lozenges equal to that constructed will accurately fit, and the entire structure will be completed. Models in cardboard have been easily made in this way, and I had one afterwards made in glass.

* As $\tan\theta=\frac{2\tan\frac{1}{2}\theta}{1-\tan^2\frac{1}{2}\theta}$ if $\tan\frac{1}{2}\theta=\frac{1}{\sqrt{2}}$, $\tan\theta=2\sqrt{2}$.

V. "Results deduced from the Measures of Terrestrial Magnetic Force in the Horizontal Plane, at the Royal Observatory, Greenwich, from 1841 to 1876." By Sir G. B. AIRY, K.C.B., F.R.S., late Astronomer Royal. Received June 24, 1885.

(Abstract.)

In offering to the Royal Society some results deduced from the systems of magnetic observation and magnetic self-registration established several years since at the Royal Observatory, Greenwich, during a portion of the time in which I presided over that institution, I think it desirable to premise a short statement on the origin of the Magnetic Department of the Royal Observatory, and on the successive steps in its constitution.

It appears to have been recognised many years ago, that magnetic determinations would form a proper part of the business of the Royal Observatory. When I commenced residence at the Royal Observatory, at the end of 1835, I found in the garden a small wooden building, evidently intended for the examination of compasses, perhaps of the size of those used in the Royal Navy. But the locality was inconvenient, and the structure was totally unfit for any delicate magnetic purpose; for instance, the balance-weights of the sliding windows were of iron. For some preliminary experiments a small observatory was borrowed from Captain Fitzroy, but no real progress was made in magnetism.

In the beginning of 1836, a scheme for the erection of a Magnetical Observatory was brought before the Board of Visitors. The Board approved the plan, and recommended it favourably to the Admiralty. The Government Department superintending the Park gave their consent to an extension of the grounds of the Observatory, and the ground was inclosed in 1837. The Magnetic Observatory was built, from my plans, in the spring of 1838. Since that time, no alteration has been made in the building, except in 1864, when the ground below the east, west, and south arms, was excavated, in order to obtain positions for the three fundamental instruments in which the severity of the temperature-changes would be much diminished. Small accidental interruptions of observations occurred in 1847, January, and 1861, July.

The interest taken in the subject of terrestrial magnetism in the first half of this century was occasioned principally by the enterprise of Gauss and other German philosophers. Magnets were, therefore, established at the Royal Observatory, furnished with apparatus adapted to eye observations corresponding to those of Gauss, and some

observations were made in concert with the Germans. The observations to the end of 1847 with these instruments were made entirely by eye; the instruments (magnets 2 feet in length) being furnished with small plane reflectors, to which telescopes were directed, and by which fixed marks were observed. The observations were made at every two hours, day and night; proper precautions were taken for assurance of the general accuracy of the times of observation; and I do not doubt that the results interpreted from these observations are each as good as those derived from the succeeding system; though the intervals of two hours were longer than I could wish. But the labour was great, and (as measured by the interruption of assistants' work) was expensive.

The idea of self-registration by photography of the movements of the intruments (an idea little entertained before that time) then suggested itself; and, at the Cambridge Meeting of the British Association in 1845, it was proposed for consideration of the Council of that body, that the Government should be requested to promote, by offer of a pecuniary reward, the construction of a photographic self-registering instrument. This proposal was adopted by the Council; letters were addressed by Sir John Herschel, President of the Association, to Her Majesty's Treasury, and by myself to the Admiralty; and, finally, the assistance of Dr. Charles Brooke was secured, for forming an efficient apparatus, and making the necessary chemical arrangements adapted to our wants.

I do not propose here to describe the photographic recording apparatus. Allusions to the construction will be found in the Introductions to the Greenwich Observations for successive years, and especially, and in great detail, in the introduction to the volume for 1847. The only alteration that was made in it for several years is the following. Mr. Brooke had conceived that advantage would be gained by making the recording barrel to revolve in twelve hours. But this caused a doubling of the curves traced on the photographic paper which is wrapped upon the barrel; and the inconvenience produced by this doubling was soon found to be so great that I thought it necessary to alter the clock-work so as to produce a revolution of the barrel in twenty-four hours. The records of the change of western declination from the north, and of the change of horizontal force, are made on the same barrel; and by alterations, first suggested by myself about 1881, and carried out by the present Astronomer Royal (then Chief Assistant), the two curves are now so traced that the simultaneous records of the two instruments at all times are in close juxtaposition.

While the observations were made by eye, at every two hours, the means of the two-hourly readings were adopted as base for the day, and the excess of each two-hourly reading above the mean was adopted

as "magnetic inequality" of that ordinate for that hour; producing twelve measures of "inequality" for each day. When the photographic system was introduced, the elevation of a pencil curve drawn by eye so as to smooth down the irregularities of the photographic trace above a photographic base was measured for every hour, producing twenty-four measures of "inequality."

In the instances of excessive and rapid disturbances of the magnets during magnetic storms, no measures of ordinates were taken for the present purposes.

Thus the daily measures at each hour or two hours were obtained.

The next step was to collect for each month all the daily measures on corresponding hours through each month, and to take their mean. These are the measures for the hours which are actually treated in the present memoir. By combining (for each month) the inequality of magnetic horizontal force at every two hours or each hour, as abscissa, with the inequality of magnetic declination (on the same scale of measure) at the same two hours or hour, as ordinate, points were defined in every monthly curve representing completely the mean diurnal changes of magnetism for each month. On the recommendation of the Board of Visitors of the Royal Observatory, reduced photographic copies of these curves were prepared by the Astronomer Royal for publication with the volume of Greenwich Observations for 1884.

The number, and the character, of the curves produced uninterruptedly on this plan, and the circumstance that they are intended for publication in the Greenwich Observations, appear to render them unfit for dissemination in the Royal Society's Transactions. I have, therefore, decided on the following course. With the permission of the Astronomer Royal, I have adopted the three years 1863, 1864, 1865, for partial exhibition of results. (Any other years would have answered equally well, for general exhibition.) For each of these years I have attached to this paper the curves for the months January, April, July, October, which suffice for showing generally the characteristic changes of magnetism for the several months. But some general account may be given, for which this is perhaps a suitable place.

The form of the curves, and the position of the points on them corresponding to hours of solar time, leave no doubt that the diurnal inequality is due mainly—and, as far as I can judge, entirely—to the radiant heat of the sun; and, it would seem, not to the sun's heat on the earth generally, but to its heat on parts of the earth not very distant from the magnets. In the hot months of the year, the curve, though far from circular, surrounds the central point in a form which, as viewed from that central point, never crosses itself; and is, roughly speaking, usually symmetrical with regard to E. and W.

But in the cold months, the space included in the curve is much smaller in many cases, probably not more than one-sixth of what it is in the summer months; and the curve often crosses itself in the most bizarre fashion with irregular loops stretching out, three crossings in one curve occurring very frequently. In the summer months there is a certain degree of symmetry; but here is, constantly, a preponderance on the west side, which leads me to imagine that the magnetic effect of the radiant heat upon the sea is considerably greater than the effect on the land.

To obtain some numerical basis for a report, which though exceedingly imperfect may convey some ideas on this wonderful subject, I have adopted the following course. I have confined myself to the months of June and July as probably the two hottest, and the months of December and January as probably the two coldest. For each of the curves applying to these months, I have laid down a system of rectangular co-ordinates, corresponding to the Greenwich meridian and to the line at right angles to the meridian (or the geographical E. and W.). The extreme north ordinate and the extreme south ordinate are measured, and their sum is taken, and interpreted by a scale of measure formed in accordance with the theory of the intruments; and this interpreted sum forms the "Range of Meridian Force" in terms of the whole Meridian Force. In the same manner, the "Range of Transversal Force" is measured. As the time of each of the two-hourly or hourly records is marked on the curve, there is no difficulty in fixing approximately on the solar times corresponding to the extreme N. and S. values, and the extreme E. and W. values, mentioned above. These are all the elements of the magnetic record which are described in the subjoined table.

The changes in the monthly records are very remarkable. They leave no doubt in my mind that the diurnal magnetic changes are produced by the sun. But I cannot account for every change that takes place in the course of a day; nor can I undertake to say whether we can found, on these, the theory that general terrestrial magnetism is a part of solar radiation, perhaps sometimes acting through or sometimes impeded by the masses of land and sea on which that radiation acts.

Still I think that a considerable step is made by the establishment of a connexion between terrestrial magnetism (on one hand), and the radiation, or, at least, the visibility of the sun (on the other hand).

VI. "Studies of Disinfectants by New Methods." By A. WYNTER BLYTH, Medical Officer of Health. Communicated by Dr. B. W. RICHARDSON, F.R.S. Received October 8, 1885.

The object of this paper is to communicate the results of a study of the action of disinfectant substances which has occupied the leisure of the author for the past eighteen months. Three series of experiments have been made, viz.:—

1. On the *Bacterium termo*.
2. On the various micro-organisms in sewage.
3. On the disinfection of typhoid excreta.

The term disinfectant as used throughout this paper must be considered as synonymous with "germicide"; to disinfect a thread or a drop of liquid contaminated by bacteria is, according to my view, to kill, and to kill not by a destructive or corrosive, but by a true poisonous action, all the micro-organisms, so that the "disinfected" micro-organisms placed under the most favourable conditions for growth are incapable of any further development.

1. *The Disinfection of the* Bacterium termo.

A pure cultivation of the *Bacterium termo* was made in ordinary sterile, solid, nutrient gelatin; the somewhat fluorescent greenish liquid to which the upper layers of the gelatin were reduced, by the growth and multiplication of the bacterium, was submitted to the various disinfectants in the manner to be described, and then the bacterium was withdrawn as far as possible from the influence of the disinfectant and planted, as it were, in fresh nutrient gelatin. The methods used in the sterilisation of beakers, test-tubes, &c., as well as the preparation of nutrient gelatin, differed in no essential respect from the same methods in general use in biological laboratories, and therefore need not be described.

The *B. termo* was submitted to the action of the disinfectants by two methods, which may be called the "drop" and "thread" method respectively.

The Drop Method.—Sterilised pure water was infected with a few c.c. of gelatin liquefied by the bacterium; measured volumes of this infected water were added to measured volumes of the disinfectant, and the whole allowed to act for twenty-four hours. A drop of this liquid was then added to from 10—20 grams of the nutrient gelatin, first liquefying it at a very gentle heat. As the proportion of the weight of the drop to the weight of the nutrient gelatin varied from about 1 : 500 to 1 : 1000, the dilution was in most cases sufficient to

reduce any antiseptic or inhibitory action of the minute quantity of the chemical agent in the drop itself to a minimum, so as to exercise no appreciable effect.

The Thread Method.—In the thread method capillary glass rods were made by drawing out ordinary glass tubing in the blow-pipe flame, these were tipped with sealing-wax, and to the wax a little bit of sterilised cotton wool was made to adhere.

The end of the rod thus prepared was infected with the bacterium by a short immersion in a pure cultivation and was then placed in the disinfectant for twenty-four hours. The rod on removal was soaked for a little time in sterilised water until all trace of the disinfectant had been removed.

The rod thus charged and purified was next inserted into a mass of solid nutrient gelatin in a test-tube, and put on one side at the ordinary temperature of the atmosphere, protected of course from external contamination by a suitable plug of sterilised wool. Whether the process used was that which I have called the "drop" or the "thread," in each case "control" experiments were made with threads infected with the bacterium, but which had not been submitted to disinfection.

Alcohol, Ether, &c.—As it was necessary to dissolve many of the substances experimented upon in weak alcohol, a series of experiments were made to ascertain the influence of alcoholic and other solvents on the life of the bacterium, and the following table gives the result. Alcohol of 60 per cent. disinfected, but absolute amyl alcohol, pure ether, chloroform, and carbon disulphide merely delayed the growth.

Alcohol, Chloroform, Ether, and Carbon Disulphide—(Thread Method).

Days........	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
At 15·5°—											
Alcohol 5 per cent. ..	+										
" 10 " ..	+										
" 20 " ..	−	+									
" 30 " ..	−	+									
" 40 " ..	−	−	+								
" 60 " ..	+	−	−	−	−	−	−	−	−	−	−
" 80 " ..	−	−	−	−	−	−	−	−	−	−	−
Carbon disulphide	−	−	−	−	−	−	−	+			
" " water 5 : 5.	−	+									
Carbon disulphide water (saturated)	−	+									
Ether	−	−	−	+							
Chloroform	−	−	−	−	+						
Control	+										

The — sign in the table denotes "no growth," the + sign means that on the day denoted in the upper column the gelatin began to liquefy, and growth to appear.

Phenol and Cresol.—These experiments were made by the "drop" method.

Weighed quantities of pure crystalline phenol and of pure liquid cresol* were dissolved in 20 per cent. alcohol in such proportion that the strength was exactly 1 per cent. Water which had been infected with the bacterium was measured from a burette into test-tubes, and definite quantities of the phenol or cresol solutions added; the volume of the whole being kept at 10 c.c., *e.g.*, 3 c.c. of phenol solution, and 7 c.c. of the infected water would equal 0·3 per cent., 1 c.c. of phenol solution and 9 of the infected water would equal 0·1 per cent., and so on. At the end of twenty-four hours nutrient gelatin was infected by means of dipping a clean recently ignited platinum wire in the liquid, and then inserting the wire for a second or two in the gelatin, which had been previously liquefied. The following short table summarises these experiments; as before, the — sign denotes no growth, the + sign denotes the first appearance of evident growth and liquefaction.

Phenol and Cresol.

Days........	2.	3.	4.	5.	6.	7.	8.	9.	10.
At 15·5° C.—									
Phenol 0·01 per cent.	—	—	—	—	+				
" 0·05 "	—	—	—	—	+				
" 0·10 "	—	—	—	—	+				
" 0·25 "	—	—	—	—	+				
" 0·50 "	—	—	—	—	—	—	—	—	—
Cresol 0·01 "	—	—	—	—	+				
" 0·05 "	—	—	—	—	+				
" 0·10 "	—	—	—	—	+				
" 0·25 "	—	—	—	—	—	—	—	—	—
" 0·50 "	—	—	—	—	—	—	—	—	—
Control	+								
At 35·5° C.—									
Phenol 0·01 per cent.	—	—	—	+					
" 0·05 "	—	—	—	—	+				
" 0·10 "	—	—	—	—	—	+			
" 0·25 "	—	—	—	—	—	—	—	—	—
Cresol 0·01 "	—	—	—	+					
" 0·05 "	—	—	—	—	+				
" 0·10 "	—	—	—	—	—	—	+		
" 0·25 "	—	—	—	—	—	—	—	—	—

* The cresol used was the purest commercial sample, and was obtained from Messrs. Calvert.

The table gives the results obtained at the ordinary temperature, and also at the temperature of 35·5°.

The effect of a temperature of 35·5° is remarkable, and goes far to explain the happy effects of the so-called antiseptic or Listerian method of surgery. The effect of the higher temperature is seen mainly in the longer period of time between the infection and the subsequent growth, *e.g.*, when the phenol was present in the proportion of 0·1 per cent., under these conditions the growth was retarded to the seventh and eighth day, and 0·25 of phenol which did not disinfect with certainty at from 15—16° did so at 35·5°.

Pyridine Series.—Pure samples of pyridine (C_5H_5N), picoline (C_6H_7N), lutidine (C_7H_9N), collidine ($C_8H_{11}N$), parvoline ($C_9H_{13}N$), and also acridine ($C_{13}H_9N$), and acridine hydrochlorate were placed at my disposal by Mr. Benjamin Nickels.

Solutions of the bases were made in 20 per cent. alcohol, and the bacterium was experimented upon on the same lines as in the pre- series of experiments.

The table gives the general results obtained, and establishes well

The Pyridine Bases.

Days........	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
At 15·52—										
Pyridine 0·9 per cent.	−	+								
" 1·6 "	−	−	−	−	−	−	−	−	−	−
" 2·0 "	−	−	−	−	−	−	−	−	−	−
Parvoline 0·45 "	−	−	−	−	−	−	−	−	−	−
" 0·83 "	−	−	−	−	−	−	−	−	−	−
" 1·15 "	−	−	−	−	−	−	−	−	−	−
" 0·25 "	−	−	+							
Picoline 0·9 "	−	+								
" 1·6 "	−	+								
" 2·3 "	−	−	+							
" 3·0 "	−	−	−	−	−	−	−	−	−	−
Lutidine 0·45 "	−	−	+							
" 0·83 "	−	−	−	−	+					
" 1·15 "	−	−	−	−	+					
At 35·5°—										
Lutidine 0·5 "	−	−	−	−	−	−	−	−	−	−
At 15°·5—										
Collidine 0·45 "	−	+								
" 0·83 "	−	−	−	−	+					
At 35·5°—										
Collidine 1·0 "	−	−	−	−	−	−	−	−	−	−
At 15°·5—										
Acridine 0·1 "	−	+								
" hydrochoride 0·01 p.c.	−	−	−	+						
At 35·5°—										
Acridine hydrochloride 0·01 p.c.	−	−	−	−	−	−	−	−	−	−

within 1 per cent. the least amount of the disinfectant which has the effect of destroying the germ life.

The order of activity seems to be as follows:—Parvoline, acridine, collidine, pyridine, lutidine, picoline.

Here also the effect of the bases at the higher temperature is very evident and marked; 1·15 per cent. of lutidine at 15·5° failed to disinfect, for growth was evident and vigorous by the sixth day; but 0·5 per cent. acting for twenty-four hours at 35·5° disinfected perfectly. Similarly, 0·1 per cent. of acridine disinfected perfectly at 35·5°, but not at the lower temperature.

It was probable enough that the empyreumatic products of tobacco, consisting to a considerable degree of members of the pyridine series, would also be disinfectant. The smoke from an ordinary pipeful of common shag tobacco was pulled through a few c.c. of sterilised water. In this tobacco-water threads infected with the bacterium were soaked for twenty-fours, and afterwards submitted to cultivation, but no growth resulted.

It may not be rash to infer that the cavity of the smoker's mouth during the act of smoking, is likely to have a disinfecting action on any bacteria which may at that time gain access.

Alkaloids.—Experiments were made on certain of the alkaloids by the "thread" method.

The exact manner in which the alkaloids were dissolved, &c., was as follows:—Brucine and strychnine were respectively converted into chlorides and the neutral salt dissolved in 20 per cent. alcohol so as to make a 2 per cent. solution.

Sulphate of atropine was dissolved in 20 per cent. alcohol. Quinine sulphate was in one experiment dissolved in 20 per cent. alcohol with the addition of a sufficient quantity of hydric sulphate to ensure solution; but in another experiment the acid was omitted, a saturated solution of the quinine being used instead; this saturated solution was made by boiling the salt for a few minutes, and then allowing the solution to cool. A solution thus made equals 0·3 per cent. of anhydrous quinine sulphate.

Morphine was used in the form of acetate. Theine was simply dissolved in the weak alcohol without further preparation.

The results are given in the table, from which it will be seen that strychnine, brucine, quinine, and atropine all destroyed the bacterium in from 0·25 to 0·5 per cent. strength. The saturated aqueous solution of quinine permitted growth on the sixth day when acting at ordinary temperatures, but when the action took place at the heat of the body then sterilisation was effected.

It is noteworthy that solutions of morphine acetate of 1 per cent. strength seem to have no disinfecting properties.

Alkaloids.

Days........	2	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
At 15° to 16·5°—											
Strychnine 0·02 p.c. ...	–	+									
" 0·04 "	–	+									
" 0·07 "	–	+									
" 0·01 "	–	+									
" 0·02 "	–	–	–	+							
" 0·25 "	–	–	–	–	–	–	–	–	–	–	–
" 0·50 "	–	–	–	–	–	–	–	–	–	–	–
" 1·00 "	–	–	–	–	–	–	–	–	–	–	–
Brucine 0·01 "	–	+									
" 0·02 "	–	+									
" 0·25 "	–	–	–	–	–	–	–	–	–	–	–
" 0·50 "	–	–	–	–	–	–	–	–	–	–	–
Quinine sulphate dissolved by means of acid in water 0·5 p.c.	–	–	–	–	–	–	–	–	–	–	–
Quinine sulphate dissolved by means of acid in water 1·0 p.c.	–	–	–	–	–	–	–	–	–	–	–
Quinine sulphate in water 0·3 per cent.	–	–	–	–	+						
At 35·5°—											
Ditto	–	–	–	–	–	–	–	–	–	–	–
At 15° to 16·5°—											
Atropine sulphate 0·5 per cent.	–	–	–	–	–	–	–	–	–	–	–
Aniline water 1 : 9....	–	+									
" 2 : 8....	–	–	+								
" 3 : 7....	–	–	–	–	–	–	–	–	–	–	–
Theine 1 per cent.	–	–	–	–	–	–	–	+			
Morphine acetate 0·5 per cent.	–	–	+								
Morphine acetate 1·0 per cent.	–	–	–	–	–	+					
Control	+										

Ferrous Sulphate.—Infected threads steeped many hours in a saturated solution of ferrous sulphate (16·7 per cent.) afterwards developed a strong growth, thus confirming other researches as to the unreliability of this salt as a disinfectant.

Potassic Permanganate.—Experiments on the action of potassic permanganate were made by the thread method. As the infected thread was immersed in a large volume of the disinfectant, the latter acted under more favourable conditions than are likely to occur in actual practice, in which there will be usually a quantity of easily broken up organic matter, decomposing the permanganate, and thus in effect removing it.

The results are given in the table, from which it appears that no

true disinfectant action takes place until the strength reaches 1 per cent.

Ferrous Sulphate and Potassic Permanganate.

Days........	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
At 16°—										
Ferrous sulphate (saturated) 16·7 per cent.	–	–	–	–	–	–	–	+		
Ferrous sulphate 8·4 per cent	–	–	–	–	+					
Ferrous sulphate 5 per cent. .	–	–	–	–	+					
Ferrous sulphate 1·6 per cent.	–	–	–	–	+					
At 35·5°—										
Ferrous sulphate 1·6 per cent.	–	–	–	–	–	+				
At 16°—										
Potassic permanganate 0·01 per cent.	–	+								
Potassic permanganate 0·04 per cent.	–	+								
Potassic permanganate 1·0 per cent.	–	–	–	–	–	–	–	–	–	–
At 35·5°—										
Potassic permanganate 0·04 per cent.	–	–	–	–	–	–	+			
Potassic permanganate 0·4 per cent.	–	–	–	–	–	–	–	+		
Control	–	+								

Halogens.—Since minute quantities of the halogens have a very decided inhibitory action on growth, the thread method of investigation was thought more suitable. Sterilised threads were, therefore, infected with the bacterium, and submitted for twenty-four hours to chlorine, bromine, and iodine water of known strength, the thread being afterwards soaked in distilled water to free it from all traces of the halogen, and then planted in gelatin. The results are not essentially different from those obtained by other observers, and fully confirm the great disinfecting power of the halogens, 0·01 per cent. solution of any of the three being sufficient to destroy the bacterium. Of the three, chlorine is the most active.

Chlorine, Bromine, Iodine.

Days........	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
Chlorine 0·001 p.c...	+											
,, 0·002 ,, ..	−	+										
,, 0·004 ,, ..	−	−	−	−	−	−	−	−	−	−	−	−
,, 0·01 ,, ..	−	−	−	−	−	−	−	−	−	−	−	−
,, 0·02 ,, ..	−	−	−	−	−	−	−	−	−	−	−	−
Iodine 0·001 ,, ..	+											
,, 0·002 ,, ..	−	+										
,, 0·004 ,, ..	−	+										
,, 0·01 ,, ..	−	−	−	−	−	−	−	−	−	−	−	−
Bromine 0·001 ,, ..	+											
,, 0·002 ,, ..	−	+										
,, 0·004 ,, ..	−	+										
,, 0·01 ,, ..	−	−	−	−	−	−	−	−	−	−	−	−
Control........	+											

2. *Experiments on the Disinfection of Sewage.*

In the following series of experiments an entirely different method of procedure was adopted. The number of colonies in a gram of sewage or other suitable liquid was carefully determined by a modification of known methods.

The same sewage was then treated by substances the disinfectant properties of which formed the subject of inquiry, and the number of colonies capable of growing in a nutrient soil, representing the micro-organisms which had escaped destruction, again enumerated.

The only special apparatus used requiring description is the "drop-bottle" and the "rings and plates."

The Drop-bottle.—The figure represents its shape and size, the capacity is about 25 c.c. The stopper is hollow and terminates in a pipette; it has a pin-hole at *a*, which can be closed by the finger.

The Glass Plates and Rings.—The glass plates were 4 by 2 inches square, the rings 4 inches in diameter, $\frac{1}{8}$ inch thick, and $\frac{1}{4}$ inch high. The plates had a ground surface the size of the ring thickness; the rings were cemented to the plates in the following manner. After heating the rings and plates in a hot air oven for many hours a little peptone gelatin was run on to the ribbon of ground surface, the ring adjusted, and the whole allowed to cool in a glass chamber formed by a small dish covered by a slightly larger one; at the bottom of the dish was some filter-paper moistened with a solution of mercuric chloride. The plates were not used until the gelatin cement had perfectly set. I should also add that the plates were ruled by means of a diamond into squares for the purpose of easy enumeration.

Solid substances, such as ferrous sulphate, were weighed and dissolved in definite quantities of the sewage; in other cases solutions of known strength were mixed with the sewage. The time during which the disinfectant acted was, as a rule, twenty-four hours.

The method of cultivation was as follows:—A small quantity, whether of diluted or disinfected sewage, was transferred to the previously cleansed and sterilised drop-bottle, the bottle and its contents carefully weighed, then by means of the pipette stopper one or two drops spotted on to the surface of the glass cell formed by the plate and ring already described; the weight of the drops was ascertained by reweighing the drop-bottle.

Ordinary nutrient gelatin liquefied at a gentle heat was run from a Lister flask into the glass cell, and mixed equally with the drops by inclining the plate in different directions. During these several operations dust was excluded as far as possible by covering the glass cell by a second glass plate, merely shifting the plate sufficiently on one side to allow the insertion of the nozzle of the Lister flask or the point of the pipette. The cells thus charged were placed in the moist chamber; the gelatin rapidly set, and at the end of from three to five days the colonies of growth were counted in the usual way and their general nature determined.

The weight of the drop or drops taken varied from 20 to 100 mgrs., the gelatin in which the drop was cultivated from 15 to 20 grams, so that the minute quantity of disinfectant contained in the drop itself was diluted from 200 to 1000 times. This amount of dilution with the comparatively weak percentages of disinfectants used would reduce the action of the disinfectant on the gelatin, the cultivating

soil—to a minimum, so that practically as soon as the micro-organisms still surviving were floated into the nutrient gelatin they were removed from the sphere of disinfectant influence.

Phenol and Cresol.—It is of importance to know the relative disinfectant powers of phenol and cresol, and for this purpose the following comparative experiment was made. Two quantities of sewage were respectively treated with phenol and cresol, so that the mixtures were equivalent to 1·9 per cent., and allowed to act for twenty-four hours; the mean of two strictly concordant experiments gave the following as the number of colonies which at the end of four days could be enumerated—

	No. of colonies per gram of the sewage taken.
Phenol	33,333
Cresol	33,410
The control	1,490,000

None of the colonies in the disinfected sewage liquefied the gelatin. The fungi which developed after the treatment with phenol were to the other colonies as 3 : 5, the fungi in the cresol experiment were to the other colonies as 1 : 7. Weight for weight, pure crystalline phenol and pure liquid cresol seem to be about equal in disinfectant power.*

In another experiment 10 grams of phenol and 10 grams of cresol were mixed respectively with 90 grams of pure calcic hydrate. 1 gram of each of these powders was digested with 100 grams of sewage for twenty-four hours. The results of the cultivation were as follows:—

	No. of colonies per gram of the sewage taken.
Phenol lime	311
Cresol lime	118
Control	1,490,000

Ferrous Sulphate.—A saturated solution of ferrous sulphate was made by boiling the crystals in water, allowing to cool, and then filtering from the deposited crystals. The strength of the solution

* Mr. J. P. Laws, F.C.S., in some recent experiments on the disinfection of the *Bacillus anthracis*, found the relative "restraining" power of phenol and paracresol to be as 2 : 3, the killing power as 2 : 5; hence he gives some superiority to cresol as regards *B. anthracis*. His samples were both crystalline, and obtained from Kahlbaum and made from the benzene and toluene sulphonates of potassium (Fourteenth Annual Report Local Government Board, Supplement—Medical Officer. London, 1885, p. 209).—December 23, 1885.

was ascertained to be equal to 16·75 per cent. of the anhydrous sulphate.

Equal volumes of the saturated solution and of the sewage were mixed together, and drops weighed out and cultivated at the end of one and twenty-four hours respectively. The results of these experiments were as follows:—

	No. of colonies per gram of the sewage taken.
Sewage containing 8·37 per cent. of ferrous sulphate acting for 1 hour ..	1,250
Sewage containing 8·37 per cent. of ferrous sulphate acting for 24 hours	572
The control..................	1,490,000

Ferric Perchloride.—A solution of perchloride of iron was added to sewage in such proportion that the mixtures of sewage and iron represented 16·4, 9·2, and 5·3 per cent. of ferric perchloride; the three mixtures were placed on one side at the ordinary temperature for twenty-four hours, weighed drops of each were then taken for cultivation.

	No. of colonies per gram of the sewage taken.
1. Sewage treated with ferric chloride 16·4 per cent....................	20,856
2. Sewage treated with ferric chloride 9·2 per cent.	35,294
3. Sewage treated with ferric chloride 5·3 per cent.	42,444
Control	1,490,000

It was noted that the colonies developed comprised all classes of micro-organisms—mucors, aspergilli, bacilli, bacteria, and micrococci having all their representatives.

The proportion of fungi to the other growths was carefully determined with the following results:—No. 1, the number of fungoid growths to the rest was as 1 : 48; in No. 2, as 1 : 5; in No. 3, as 1 : 4.

Zinc Chloride.—In the same sewage which formed the subject of the previous experiments zinc chloride was dissolved, and the solutions allowed to act for twenty-four hours. The following table gives the strength of the solutions, and the number of colonies which were enumerated after four days' cultivation.

	No. of colonies per gram of the sewage taken.
Sewage containing 5·22 per cent. zinc chloride	6206
Sewage containing 11·75 per cent. zinc chloride	5764
Sewage containing 15·67 per cent. zinc chloride	1333

Mercuric Chloride.—Equal volumes of a 0·1 per cent. solution of mercuric chloride and sewage were mixed together and allowed to act for twenty-four hours.

To another portion of the sewage mercuric chloride was added so as to be in the exact proportion of 0·1 per cent.; this also was allowed to act for twenty-four hours.

The result was as follows:—

	No. of colonies per gram of the sewage taken.
Sewage containing 0·1 per cent. of mercuric chloride	550
Sewage containing 0·5 per cent. of mercuric chloride	77
Sewage undisinfected	1,490,000

Chloride of Lime.—A gram of chloride of lime of average quality was added to 100 grams of sewage and placed in an incubator set at 37°; after twenty-four hours weighed drops of the sewage were cultivated.

At the same time 2 grams of chloride of lime were added to 100 grams of the sewage and digested for twenty-four hours, at the ordinary temperature, and cultivated side by side with the above. In each the sewage had a very distinct odour of chlorine.

The result of the cultivation was as follows:—

	No. of colonies per gram of the sewage taken.
Chloride of lime 1 gram, sewage 100 grams at 37°	46
Chloride of lime 2 grams, sewage 100 grams at 17°	1638

Aniline.—1 gram of sewage was added to 99 grams of saturated aniline water, allowed to stand at the ordinary temperature of the atmosphere for twenty-four hours, and then drops weighed out and cultivated.

On the fourth day of cultivation the colonies produced were calculated to be equal to 33,846 per gram of the original sewage, which when not disinfected yielded 1,490,000 per gram. It was noted that the fungi were to the other organisms as 4 : 7. About 1500 of the colonies were of the class that liquefy the gelatine.

Quinine Sulphate.—A saturated solution of quinine sulphate was made by boiling the crystals with water, allowing to cool, and filtering from the crystals which separated. The strength of this solution was ascertained by evaporating 10 c.c. to dryness in a tared platinum dish. It was found in this way to be equal to 0·3 per cent. of the anhydrous sulphate.

1 gram of sewage was mixed with 50 grams of the saturated solution, and after acting for twenty-four hours drops were weighed out and cultivated.

The colonies developed at the end of five days were noted to be about one-half composed of fungi, and to equal 48,936 per gram of the original sewage, which, as previously stated, contained 1,490,000 per gram.

Terebene.—20 c.c. of terebene were added to 100 c.c. of sewage and digested with frequent agitation for twenty-four hours at the common temperature. In this way the sewage was saturated with as much terebene as the conditions would allow, the excess separating and floating in an upper stratum. A small quantity of the lower liquid was transferred to the weighing bottle, and some drops weighed out for cultivation.

At the end of four days the number of colonies were only equal to 83 per gram of the original sewage, nor did any fresh colonies make their appearance even after keeping the cell in the moist chamber for ten days.

Potassic Permanganate.—Two flasks, each containing 1 gram of crystallised permanganate, were submitted for twenty-four hours to different temperatures, viz., the one at 18°, the other in an incubator set at 37°.

The sewage used was the same as in the former experiments, and contained over a million and a quarter of centres of growth when cultivated in its normal state.

The sewage and permanganate which had been placed at the lower temperature yielded on cultivation colonies of micro-organisms equal to 171 to the gram, but that which had been incubated at 37° showed no sign of growth.

Ammonia, hydroxylamine, methylamine, ethylamine.—Solutions of ammonia, hydroxylamine, methylamine, and propylamine were made twice the strength of normal; that is to say, that double the equivalent of ammonia (17), of hydroxylamine (33), &c., was dissolved in a litre of water. To one volume of each of these solutions one

volume of sewage was added, and the whole allowed to rest at the ordinary temperature for twenty-four hours, at the end of which time drops of each were weighed out and cultivated in the usual manner. The control was the same sewage diluted with an equal bulk of sterilised water.

The results were as follows:—

	No. of colonies per gram of the sewage taken.
Hydroxylamine	50
Methylamine	180
Ethylamine	181
Propylamine	237
Ammonia	257
Control	6250

3. *The Action of Disinfectants on Typhoid Excreta.*

Eberth, Klebs, and Gaffky have each described micro-organisms which they consider peculiar to abdominal typhus, *i.e.*, enteric or typhoid fever. Gaffky has specially studied the question, and describes, with great minuteness, the manner of growth of a bacillus which he found in twenty-six out of twenty-eight typhoid bodies (Gaffky, "Zur Oetiologie des Abdominal Typhus." "Mittheilungen aus dem Kaiserlichen Gesundheitsamte," 2 Band, Berlin, 1884). The bacillus grows in nutrient gelatin, in light brown colonies, which do not liquefy the gelatin; if a minute portion of one of the colonies be taken up on a needle and transferred to a drop of water for microscopic observation, the bacillus is seen to have the power of self-movement. If the bacillus is sown on sterlised potatoes, a peculiar sort of pellicle in about forty-eight hours is produced, formed wholly of bacilli. The method of growth on gelatin, on potato, and the power of self-movement, taken together, Gaffky considers to belong to no other bacillus hitherto described. At the ordinary temperature no spores were formed, but they were readily produced when the bacillus was cultivated on potato at a blood heat.

Gaffky, although seldom failing to find this bacillus in typhoid bodies, could not detect it in the excreta.

In the study of the action of disinfectants on typhoid excreta, it was necessary to first determine the number of colonies which could be raised by cultivation from a gram of the typhoid matter operated upon, and also search for any distinctive organism.

From a typical case of typhoid fever, on the tenth day of the disease, a small quantity of the typhoid stool was obtained; it was very liquid, offensive, of a light brown colour, and free from blood. A portion of this was weighed and diluted with sterilised water, so

that the solution equalled 5 per 1000. Weighed drops of this solution were cultivated, and the number of colonies obtained enumerated. According to the mean of four experiments the number per gram of colonies of all kinds in the original typhoid stool was 1,031,250.

Of these various growths 40 per cent. were forms of mucor and aspergillus, about 20 per cent. were bacilli, bacteria, and micrococci, which, from their manner of growth and general characters, seemed to belong to common and familiar forms, and were not farther investigated.

Besides these there were some light brown colonies which grew slowly, the one generally in almost circular spheres, the other in flatter irregular wart-like masses; neither of these growths while cultivated on the thin sheet of gelatin-peptone in the glass cell seemed to liquefy the gelatine, that is, within five or six days of cultivation, for observation could not be carried on longer than this period, other common organisms, such as *B. termo*, liquefying the whole mass and mixing up the colonies. It was, therefore, necessary for the farther study of these brown colonies (which might be Gaffky's bacillus) to obtain of each pure cultivations. For this purpose a minute quantity of each was transplanted into a test tube of nutrient gelatin by means of a sterilised platinum wire; from this cultivation a second was produced, and from the second a third. As the general behaviour of the various cultivations never altered, nor could any foreign element be detected, the last cultivations were considered to be pure, and from these the surface of sterilised slices of potatoes were inoculated. The bacillus, which may be referred to as bacillus (*a*), and which grew in a more or less spherical manner in the sheet gelatin, when transferred to a test-tube of solid gelatin-peptone, liquefies very slowly the gelatin, growing always in contact with the air. On potato it extends as a dirty scum. The bacilli examined in water showed no power of self-movement. The irregularly growing brown colony, which may be called bacillus (*b*), also very slowly liquefies the gelatin, but only along the track of the needle. A test-tube cultivation at the end of from fourteen to twenty days has the following appearance. Along the track of the needle there is a cone-shaped perfectly liquid mass. On the surface of the liquid float little detached white-brown colonies; at the bottom is a white deposit composed of colonies which, having been formed on the surface in contact with the air, have slowly sunk. The liquid intervening between the deposit and upper floating colonies is perfectly clear. On potato the bacillus grows rapidly, forming an irregularly-shaped brownish crust, and ultimately presents an appearance very similar to the common wall lichen (*Parmelia parietina*).

Whether these bacilli have any connexion with the typhoid state or not, the characters described are quite different from those of Gaffky's bacillus.

Whatever their significance may be, they were for this particular sample of excreta distinctive, and moreover had such a special manner of growth that they could be readily recognised by the unaided sight, so that their presence or absence in the succeeding experiments could be easily noted.

The disinfectants first experimented upon were some in common use, such as ferrous sulphate, cresol, and potassic permanganate.

Ferrous Sulphate.—To the 0·5 per cent. typhoid water crystals of the sulphate were added and allowed to remain at the ordinary temperature for twelve hours, crystals at the end of that period were still undissolved, so that in effect the solution was saturated.

Of this liquid a quantity equal to 125 mgrms. of the original typhoid stool was taken for cultivation. Growth was rapid; at the end of three days fifteen colonies were counted, composed of common liquefying bacteria and bacilli; besides these, there were sixty-five others which did not liquefy the gelatine, *i.e.*, within the same time, among which were to be found both kinds of the light brown bacilli above described. The total number of colonies calculated per gram of the original typhoid matter, which had escaped destruction, were thus 640.

Cresol.—The crude carbolic acid of commerce, so largely used for disinfecting purposes, is a mixture of phenol and cresol with small quantities of other tar principles. The cheapest carbolic acid may be considered impure cresol, all phenol that the manufacturer can possibly crystallise out having been removed; hence in the following experiments with Calvert's cresol some information is obtained as to the value of the disinfection of typhoid matters as ordinarily performed.

An exact 1 per cent. solution of cresol was made in typhoid water, and drops weighed out, at the end of fifteen minutes, three hours, and twenty-four hours respectively.

The number of colonies per gram of the original typhoid matter appearing at the end of four days' cultivation was as follows :—

	No. of colonies per gram of typhoid matter taken.
Cresol acting for 15 minutes	89
" " 3 hours	27
" " 24 "	8

The brown colonies were not detected in the last cultivation, but were present in the others.

The experiments were next extended to the amines and to the pyridine series, substances, the action of which on *Bacterium termo* and on sewage had already been studied.

Ammonia, hydroxylamine, and the amines.—The same strength of solution of ammonia and the amines employed in the treatment of sewage was also used in the experiments on typhoid matter.

The 0·5 per cent. of typhoid stool was diluted with an equal volume of these solutions, with of course the result that every 100 parts contained by weight one-tenth of an equivalent of ammonia, hydroxylamine, and the amines, and 0·25 per cent. of typhoid matter.

As a control one volume of typhoid water was diluted with one volume of sterilised water and cultivated side by side with the others. At the end of four days the colonies were enumerated, the light brown bacilli (*a*) and (*b*) were present as well as representatives of all others seen in the control.

	No. of colonies per gram of typhoid matter taken.
Methylamine	14,350
Hydroxylamine	22,222
Ethylamine	33,333
Propylamine	73,913
Ammonia	103,703
The control	500,125

The order in which the different amines stand is pretty well the same as in the similar experiment on the disinfection of sewage (see *ante*), but none of them seem to be strong disinfectants.

The Pyridine Series.—2 per cent. solutious of pyridine, C_5H_5N, picoline, C_6H_7N, lutidine, C_7H_9N, and parvoline, $C_9H_{13}N$, were made in 20 per cent. alcohol. Equal volumes of these solutions were then mixed with equal volumes of the 0·5 per cent. typhoid water, and after the end of twenty-four hours weighed drops were cultivated.

Pyridine.—The quantity taken for cultivation of the typhoid water was equivalent to 0·5 mgrm. of the typhoid stool. After four days' cultivation three colonies developed, two of these were common moulds, the third a common bacillus.

Picoline.—After four days' cultivation, a quantity of the solution equivalent to 0·885 mgrm. of typhoid yielded fifteen colonies, five of which were identical with the light brown bacillus (*a*) already described.

Lutidine.—After four days' cultivation, a quantity of the solution equivalent to 0·62 mgrm. of typhoid stool yielded twelve colonies, all of which seemed of a common kind.

Parvoline.—After four days' cultivation, a quantity of the solution equivalent to 0·85 mgrm. of typhoid stool yielded five colonies, five of these were common forms of mould, one was the light brown bacillus (*b*) previously described.

Hence with regard to the members of this series experimented

upon, the number of colonies per gram of typhoid developed in a normal solution acting for twenty-four hours was as follows:—

	No. of colonies per gram of typhoid matter taken.
Parvoline, $C_9H_{13}N$	5,882
Pyridine, C_5H_6N	6,000
Picoline, C_6H_7N	16,949
Lutidine, C_7H_9N	19,355

Summary.—From the three series of experiments certain general conclusions may be drawn; these are as follows:—

1. The relative merits of phenol and cresol as a disinfectant are fairly equal, as shown by experiments on *Bacterium termo* and on sewage, so that preference for one or the other must be determined from considerations apart from degrees of activity.*

2. Ferrous sulphate as a disinfectant of *Bacterium termo*, of sewage, and of typhoid excreta is shown to be unreliable. Even strong solutions fail to destroy all classes of micro-organisms. Considering the extensive use of ferrous sulphate in cases of typhoid fever, and that in both scientific and popular manuals ferrous sulphate is confidently recommended as a disinfectant of specific excreta, it seems important to accentuate the fact that my experiments are in their result wholly opposed to the popular view and custom.

3. The experiments on the amines clearly show that the disinfectant action of members of that series differs in degree according to the displacement of hydrogen by methyl, ethyl, propyl, or hydroxyl. The connexion between chemical constitution and disinfectant action is also seen in the experiments on the pyridine series, but is not so marked.

4. Other things being equal, the shorter the time a disinfectant acts, the less the disinfection; this was shown very clearly in the experiments on typhoid excreta treated with cresol for varying periods of time. It, therefore, necessarily follows that even when strong disinfectants are poured on to specific excreta, and the whole within a few minutes thrown into a drain or cesspool, which by great dilution more or less removes the excreta from the sphere of disinfectant influence, no true disinfection has been accomplished.

5. Disinfection is far more efficient at 35·5° to 37°, the temperatures at which development and growth of micro-organisms is most active, than at ordinary temperatures. This is shown in the experiments on *Bacterium termo* with phenol, cresol, lutidine, collidine, and potassic permanganate, as well as in the experiments in the treatment of sewage with chloride of lime and potassic permanganate.

* See foot-note on p. 268.

November 30, 1885.

ANNIVERSARY MEETING.

Professor T. H. HUXLEY, D.C.L., LL.D., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Society was presented, by which it appears that the total receipts during the past year amount to £6,134 7*s.* 2*d.*, and that the total expenditure in the same period amounts to £4,918 18*s.* 11*d.*, leaving a balance of £1,183 1*s.* 3*d.* at the Bankers', and £32 7*s.* 0*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary.

On the Home List.

Andrews, Dr. Thomas.
Arnott, James Moncrieff, F.R.C.S.
Barlow, Peter William, F.G.S.
Carpenter, William Benjamin, C.B., M.D.
Davidson, Thomas, LL.D.
Field, Frederick, F.R.S.E.
Flight, Walter, D.Sc.
Guy, William Augustus, M.B.
Houghton, Richard Monckton Milnes, Lord, M.A.
Jeffreys, John Gwyn, LL.D.
Jenkin, Henry Charles Fleeming, M.I.C.E.
Montefiore, Sir Moses, Bart.
Pole, William, M.A.
Selkirk, Thomas James, Earl of, F.G.S.
Teignmouth, Charles John Shore, Lord, D.C.L.
Vaux, William Sandys Wright, M.A.
Voelcker, Augustus, Ph.D.
Watson, Morrison, M.D.
Weldon, Walter, F.C.S., F.R.S.E.
Yolland, William, Colonel, R.E., C.B.

On the Foreign List.

Henle, Franz Gustav Jakob.
Milne-Edwards, Henry.
Siebold, Karl Theodor von.

Changes of Name and Title.

Northcote, Right Hon. Sir Stafford Henry, *to* Earl of Iddesleigh.
Roberts, William Chandler, *to* Roberts-Austen.

Fellows elected since the last Anniversary.

Baird, A. W., Major R.E.	Hicks, Henry, M.D.
Bowen, Right Hon. Lord Justice Sir Charles Synge C., Kt.	Hicks, William Mitchinson, M.A.
Carpenter, Philip Herbert, D.Sc.	Japp, Francis R., Ph.D.
Clark, Sir Andrew, Bart., M.D.	Marshall, Arthur Milnes, M.D.
Common, Andrew Ainslie, F.R.A.S.	Martin, Prof. Henry Newell, D.Sc.
Creak, Ettrick William, Staff Commander R.N.	O'Sullivan, Cornelius.
Divers, Prof. Edward.	Perry, Prof. John.
	Ringer, Prof. Sydney.
	Vines, Sidney Howard, D.Sc.

Re-admitted.

James Bateman.

On the Foreign List.

Cornu, Alfred. | Dana, James Dwight.

The President then addressed the Society as follows:—

At the earliest opportunity after my return to England last spring, I offered my very grateful acknowledgements to the Society for the kindness with which the Fellows had condoned my enforced absence from my post during the winter. And I should not venture to occupy your time by recurring to the subject, did not the return of St. Andrew's day admonish me that duty and inclination alike require me to offer my especial thanks to the Treasurer for the cheerful readiness with which he took upon himself the burden of my duties, and the efficiency with which he discharged them on our last Anniversary.

On the last occasion on which I had the honour to address you, it was my painful duty to commence by lamenting the death of a very eminent member of the Society, who was, at the same time, one of my oldest and most intimate friends. I deeply regret to find myself once more in this position. The lamentable accident which has deprived the Society of one of its oldest and most distinguished Fellows, Dr. Carpenter, has robbed me of a friend, whose kindly sympathy and help were invaluable to me five-and-thirty years ago, and who has never failed me since.

You are all acquainted with Dr. Carpenter's great and long continued services to science as an investigator and as an expositor of remarkable literary skill; and there must be many here who, having worked with him in the University of London, of which he was so long Registrar, are familiar with the high integrity, the energy, and the knowledge, which marked him as an administrator. He was a man of varied accomplishments outside the province of science, single-minded in aim, stainless in life, respected by all with whom he came in contact.

Within the last few days, Physics has lost an eminent representa-

tive in Dr. Thomas Andrews, of Belfast. Among the cultivators of Chemical Science we have to regret the decease of Mr. Field, who was one of the original members of the Chemical Society; of Mr. Weldon, and of Dr. Voelcker, whose names are well known in connexion with manufacturing and agricultural chemistry. In Biology, we have lost Dr. Davidson, whose elaborate monographs on the fossil Brachiopoda are remarkable examples of accurate malacological work combined with artistic skill; Dr. Gwyn Jeffreys, the veteran explorer of our marine molluscous fauna, and a high authority on conchology; and Dr. Morrison Watson, whose early death has cut short the career of an anatomist of much promise. Mineralogy has suffered a similar loss by the premature death of Dr. Walter Flight. In Engineering Science, we have to lament the deaths of Mr. Barlow and Professor Fleeming Jenkin. I may be permitted to dwell for a moment upon the latter name, as that of a most genial and accomplished man and a valued personal friend, with whom it had been my privilege to be associated for a time in his well-directed and successful efforts to improve the sanitary condition of our cities. The elder generation of English geologists will remember the keen interest which the Earl of Selkirk took in their pursuits. The death of Lord Houghton robs us of a connecting link with all the world.

Three very distinguished names have disappeared from the ranks of our foreign members: that of Henle, of Göttingen, among whose many merits must stand that of ranking next after Schwann among the founders of histology; that of the venerable Henry Milne-Edwards, of Paris, one of the most distinguished members of the school of Cuvier, and admirable no less for his contributions to zoological philosophy than for the extent and the precision of his additions to our knowledge of facts; and lastly, that of Von Siebold, of Münich, whose remarkable investigations into the phenomena of parasitism and of sexless reproduction brought about the solution of some of the most difficult problems of zoology, while it would be difficult to exaggerate the influence of his wonderfully accurate and comprehensive "Handbook" on the progress of invertebrate zoology forty years ago.

On the 1st of December last year the total number of Fellows of the Royal Society amounted to 519; of these 473 were on the home and 46 on the foreign list. Deducting Her Majesty, our Patron, and four other Royal personages, the number on the home list was 468. At the present moment, we have 45 foreign members; while the total strength of the home list (deducting Royal personages) is 465, or three fewer than twelve months ago. The number of deaths in the home list during the past year is 20. This is a larger mortality than that of last year; and it still exceeds the number of Fellows added to the Society by election, which during the past year was 16;

namely, the statutory 15 Fellows elected in the ordinary way and 1 Privy Councillor.

As the Treasurer observed in his address on the last Anniversary, it is obvious that we are rapidly approaching a state of equilibrium between our losses and our gains; and under the present conditions of election, the strength of the home list may be expected to remain somewhere between 460 and 470.

While our number thus tends to remain stationary, the list of candidates for the Fellowship, though it has fluctuated a good deal from year to year, has, on the whole, become longer, until, at present, the candidates are more than four times as numerous as the annual elections sanctioned by our rules. This state of things has given rise to comment, both within and without the Society, on more than one occasion. It has been said that any restriction upon the number of our Fellows is unwise, inasmuch as we narrow our influence and diminish our revenues thereby; and, by way of a still more unpleasant suggestion, it is hinted that, by such limitations, we lay ourselves open to the charge of a desire to arrogate to ourselves the position of the elect of science.

With respect to the first objection I venture to point out, that the influence of the Society upon the advancement of science is not by any means measured either by its numerical strength or by the amount of the funds at its disposal.

And, as to the second charge or insinuation, if it is worth while to meet it at all (which may be doubtful), I am disposed to think that, in another than the invidious sense of the words, it is highly desirable that the Fellows of the Royal Society should regard themselves, and be regarded by others, as the elect of science. An organisation which was the direct product of the new birth of science in the days of Gilbert, of Galileo, and of Harvey; which was one of the earliest of the associations founded for the sole purpose of promoting natural knowledge; and which has so faithfully performed its functions that it is inseparably associated with all the great strides which science has made for two centuries, has insensibly and without effort become a recognised representative of men of science in these islands: as such, on the one hand, it is consulted by the Government on scientific questions; and, on the other hand, it claims the right to be heard by the Government on all questions of scientific interest. I believe it to be impossible that the Society should discharge the functions which it has not sought, but which have thus devolved upon it, satisfactorily, unless it really does consist, in one sense, of the elect of science; that is to say unless every care is taken to keep its scientific character at the level of its scientific reputation, and to ensure that it shall be not the mere figure-head of the scientific body, but a living association of representative men engaged in all branches of scientific activity.

Those among my hearers whose memories go back forty years will remember that, at that time, the Society was in great danger of losing its scientific character, though it would doubtless have taken it a long time, and a good deal of perversity, to get rid of its scientific reputation. It had become the fashion to append F.R.S. to a name, and the scientific members were in danger of being swamped by the invasion of *dilettanti*. The aim of our eminent colleague, Sir William Grove, and his friends, who fought the battle of 1847, and thereby, to my mind, earned the undying gratitude of all who have the interests of science at heart, was not to create an academy of immortals, but to save the Fellowship of the Society from becoming a sham and an imposture. And they succeeded in their object by carrying a measure of reform which embodied two principles—the first, that of the practical responsibility of the Council for the elections, the second, that of the limitation of the number of candidates annually elected. The result of the steady adherence of the Society to these principles for thirty-eight years is that, year by year, the Society has approached more and more closely to that representative character which, I cannot but think, it is eminently desirable it should possess.

During a great part of this time I have enjoyed more and closer opportunities than most people of watching the working of our system. Mistakes have been made now and then, no doubt, for even members of Council are fallible; but it is more than thirty years since the propriety of the selections made by the Council has been challenged at a general meeting; and I have never heard a question raised as to the conscientiousness with which the work is done, or as to the desire of the Council to mete out even-handed justice to the devotees of all branches of science. I am very strongly of opinion that if the Royal Society were a "Chamber of Science," subject to dissolution, and that after such dissolution a general election, by universal suffrage of the members of all scientific bodies in the kingdom, took place, an overwhelming majority of the present Fellows would be re-elected.

Such being my conviction, it is natural that I should express a fervent hope that the Society will never be tempted to depart from the principles of the method by which, at present, it recruits its strength. It is quite another question, however, whether it is desirable to retain the present limit to our annual addition or to increase it.

There is assuredly nothing sacred in the number 15; nor any good reason that I know of for restricting the total strength of our home list to 460 or 470; so long as our recruits approve themselves good soldiers of science the more we enrol the better. And if I may pursue the metaphor, I will add that I do not think it desirable that our corps should consist altogether of general officers. Any such

exclusiveness would deprive us of much useful service, and seriously interfere with the representative character in which our strength lies. I think we ought to be in touch with the whole world of science in the country, and constitute a microcosm answering to that macrocosm. Those who are in favour of making a change observe that the limit of fifteen was fixed nearly forty years ago; that the number of those who occupy themselves seriously with science and attain a position which would undoubtedly have brought them into the Society at that time, has increased and is constantly increasing; and that it is undesirable that we should be compelled to leave out of our body, year after year, persons whom we should be very glad to see in it. On the other hand, it is to be recollected that a change once made can hardly be revoked, and that, in view of the importance of such a step, the Society will do well to make sure of the consequences before taking it.

I have thought it desirable to raise the question, not for the purpose of suggesting any immediate action—for my personal opinion is that, at present, no change is desirable—but in order that the attention of the Fellows may be directed to a matter which I think is sure to come before them in a practical shape before many Anniversaries go by. And, whenever that time arrives, I think another problem may possibly offer itself for solution. Since this Society was founded, English-speaking communities have been planted and are increasing and multiplying in all quarters of the globe—to use a naturalist's phrase, their geographical distribution is "world wide." Wherever these communities have had time to develop, the instinct which led our forefathers to come together for the promotion of natural knowledge has worked in them and produced most notable results. The quantity and quality of the scientific work now being done in the United States moves us all to hearty admiration; the Dominion of Canada, and our colonies in South Africa, New Zealand and Australia, show that they do not mean to be left behind in the race; and the scientific activity of our countrymen in India needs no comment.

Whatever may be the practicability of political federation for more or fewer of the rapidly growing English-speaking peoples of the globe, some sort of scientific federation should surely be possible. Nothing is baser than scientific Chauvinism, but still blood is thicker than water; and I have often ventured to dream that the Royal Society might associate itself in some special way with all English-speaking men of science; that it might recognise their work in other ways than by the rare opportunities at present offered by election to our foreign Fellowship, or by the award of those medals which are open to everybody; and without imposing upon them the responsibilities of the ordinary Fellowship, while they must needs be deprived of a large part of its privileges. How far this aspiration of mine may be reciprocated by

our scientific brethren in the United States and in our Colonies I do not know: I make it public, on my own responsibility, for your and their consideration.

I am anxious to call the attention of the Fellows to an alteration in our rules, in virtue of which it is hoped that the valuable library of the Society will be made more extensively useful to them by being accessible up to a later hour than heretofore, and by better provision for the comfort and convenience of those who desire to read or write in the Society's rooms.

The funds of the Society have been augmented in various ways during the past year.

A few weeks ago, our distinguished colleague Sir William Armstrong wrote to me proposing to make a very handsome, indeed, I may say, munificent contribution to our finances. The precise form which the gift should take was left open to discussion, and it was only at the meeting of Council to-day that the matter was finally settled by the letter addressed to me, on behalf of Sir W. Armstrong, by Captain Noble, which I proceed to read to the Society:—

"Referring to the communications which have passed between yourself and Sir W. Armstrong with regard to a proposed gift by Sir William to the Society, I have had some discussion with Sir W. Armstrong as to the form which his proposed donation should take.

"Sir W. Armstrong is very anxious that the Scientific Relief Fund Trust should be raised from £7,600, the amount at which I think it now stands, to £20,000, and he has authorised me to say that if the Fellows, with the assistance if necessary of other friends of science outside of the Society, see their way to raise £6,500, he will himself give a second £6,500, thus making up the desired sum.

"The only condition, or rather request, that he makes in connexion with this gift is that, in the event of any Fellow being in such circumstances as to make the payment of his subscription burdensome to him, the Council for the time being will order the payment of such subscription out of the income arising from the Relief Fund."

The President and Council have accepted Sir William Armstrong's generous gift, and have offered him their hearty thanks for it; they have already been promised contributions by other Fellows of the Society, and they trust that there will be no difficulty in making up the sum required.

The value of the fee for the Croonian Lecture has been increased from £2 18*s.* 9*d.* to about £50 a-year, by the falling in of certain leases.

Allusion was made in the Treasurer's Address last year to the

Darwin Memorial. I am happy to say that Mr. Boehm's admirable statue was formally and publicly accepted by H.R.H. the Prince of Wales, on behalf of the Trustees of the British Museum, last summer, and now adorns the entrance hall of the Natural History Museum at South Kensington. The balance of the sum raised, amounting to £2,000, has been handed over to the Royal Society, and the interest thereof will be employed under the name of the "Darwin Fund for the Promotion of Biological Research" in any way the President and Council may think fit. I sincerely trust that this fund may be increased from time to time, as the Donation Fund, founded by Dr. Wollaston, has been; and that its beneficent influence on the progress of biological science may thus keep green the memory of the great man whose name it bears, in the way which, assuredly, would have been most agreeable to himself.

I am sure that I may express your acknowledgments to Mr. James Budgett for the repetition of his liberal donation of £100 in aid of the cost of publication of Professor Parker's important and elaborate monographs on the vertebrate skull, one of which occupies a whole part of the Transactions, and is illustrated by 39 quarto plates.

We are indebted to the subscribers to the Henry Smith Memorial for the marble replica of the bust by Mr. Boehm of that eminent mathematician and most accomplished scholar, which now ornaments our library. The Fine Art Society has presented Mr. Flameng's etching of the portrait of your President, painted by the Hon. John Collier.

Among the presents to the Library, I may particularly mention the second volume of Professor G. Retzius' valuable and splendidly illustrated work "Das Gehör-organ der Wirbelthiere"; and "Les Habitants de Surinam," by the Prince Roland Bonaparte, by their respective authors; and four volumes of the "Challenger" Report, by Her Majesty's Stationery Office.

Five numbers of the "Proceedings" (about 880 pages) have appeared since the last Anniversary. Only one part of the Philosophical Transactions has been as yet published; but two other parts (Parts I and II for 1885) are passing simultaneously through the press.

The possibility of devising means by which papers read before us may be published more rapidly, has seriously engaged the attention of the Officers of the Society, and I trust that, before long, the Council may have some well-conceived plan for achieving that end brought under their consideration. While all will agree in deprecating unnecessary retardation, it must be remembered that a certain delay is absolutely necessary, if the Committee of Papers is to discharge with due care its important function of arriving at a sound judgment, after considering the opinions of responsible specialists on the merits of each paper submitted to it. In substance, I do not think that we

can hope to better our present arrangements; all that can be asked is that they should be improved in some details, and more especially that the time which necessarily intervenes between presentation and publication should be minimised.

The preparation of copy for the Catalogue of Scientific Papers, decade 1874—85, now approaches completion. A total of 290 series have been indexed, giving 85,000 title slips, written, checked, and distributed. This number, which is within 10,000 of that contained in the two volumes of the preceding decade, nearly exhausts the material in our own library; it remains to supplement this by reference to other libraries.

At the meeting on the 18th of June last, our Fellow, Professor Roy, communicated to the Council the project entertained by himself, Dr. Graham Brown, of Edinburgh, and Mr. Sherington (G. H. Lewes Student, Cambridge), of proceeding to Spain with a view of investigating the nature of cholera, and requested the assistance of the Royal Society.

In view of the great practical importance of such an investigation, and the desirableness of making a new attempt to solve a problem about which highly competent inquirers have arrived at contradictory results, the President and Council resolved to do everything which lay in their power to assist Dr. Roy and his colleagues. The Secretary was instructed to inquire of the Spanish Minister whether the proposed investigations would be agreeable to the Spanish authorities, and whether Dr. Roy might expect to obtain facilities and assistance. On the receipt of a courteous and sympathetic letter from his Excellency, the Secretary was further instructed to inform the Foreign Office of Dr. Roy's expedition, and to request that Her Majesty's Government would afford him and his colleagues all the assistance in their power. Moreover, £150 was granted from the Donation Fund in aid of the expenses of the undertaking, which were shared between the Royal Society and the Society for the Advancement of Medicine by Research.

I am sure the Fellows of the Society will join with me in congratulating Dr. Roy, Dr. Brown, and Mr. Sherington on having returned safe and sound from an adventure in which the interest of scientific inquiry must have been heightened by a considerable spice of personal danger. Dr. Roy has furnished me with a brief preliminary report of the work done, the substance of which I proceed to lay before the Society.

The members of the Commission met with very serious difficulties in their attempts to study the pathology of cholera in Spain, where they spent three months; but owing to the powerful support which was given them by the English Embassy in Madrid, they were

able eventually to pursue their studies in a satisfactory manner. At Aranjuez and Madrid they obtained free access to the cholera hospitals, and made nearly thirty autopsies of typical cholera cases within very short periods after death. From all of these cases they were able to obtain material for cultivation and thus to make a large series of investigations on the different forms of micro-organisms which are found in the tissues and intestinal contents of cholera cases. Owing, however, to the impossibility of obtaining animals for inoculation, and reagents of various kinds, they were unable to complete their inquiry in Spain, and they were obliged to leave the investigations of certain points until their return to England. They have directed their attention chiefly to the relation which the comma bacillus, first described by Koch, bears to the cholera process, and they hope to be able to make important additions to our knowledge of this important subject. They are at present engaged in completing their work, and in the course of a few weeks they hope to be able to present their report to the Royal Society.

The Marine Biological Association, to the funds of which the Royal Society made a substantial contribution last year, is making good progress. A site for building has been granted by the War Office, at Plymouth; plans have been prepared, and if the Treasury will follow the precedent which it has so largely and beneficially adopted in educational matters, of helping those who help themselves, as I am glad to say my Lords seem inclined to do, I trust that, before long, the laboratory will be in working order.

The prosecution of the borings in the Delta of the Nile, to which reference has been made on previous Anniversaries, have unfortunately been hindered by various obstacles. Quite recently I have been favoured by Colonel H. Maitland, R.E., with an account of the borings made near Rosetta, in which a depth of 153 feet was reached without apparently attaining the bottom of the fluviatile deposits; and I hope that circumstances may shortly permit the resumption of the original project of carrying a line of borings across the Valley of the Nile on the parallel of Tantah or thereabouts.

In the meanwhile the Committee in charge of the investigation has presented a report by Professor Judd on the results of the examination of the borings formerly made. I have been favoured by Professor Judd with the following brief summary of these results, which have been fully set forth in a paper read at the first meeting of the Society after the recess.

Although two of the recent borings in the Nile Delta have attained depths of 73 and 84 feet respectively, yet neither of them has reached the rocky floor of the old Nile Valley, nor, indeed, have they afforded

any indications of an approach to the solid rock. The samples of the Delta deposits obtained by these boring operations are found to be in all cases mixtures, in varying proportions, of Nile mud, or material carried in suspension by the river and desert sand, or particles swept in from the surrounding districts by the action of winds. The study of these materials by the aid of the microscope has revealed a number of facts which may be made the basis of generalisations of considerable interest to geologists.

The minerals present in these sands and muds are found to be such as characterise the granitic, and highly crystalline metamorphic rocks; there can be litle doubt, therefore, that the vast regions included within the Nile basin are in the main composed of rocks belonging to those classes, or of sedimentary deposits derived directly from them.

Of still greater interest is the fact that the fragments of felspars and other complex silicates in the Delta deposits exhibit but slight evidence of kaolinisation or other chemical change. This points to the conclusion that, in the rainless districts drained by the Nile, the disintegration of rocks is effected by mechanical rather than by chemical agencies. A very striking confirmation of this conclusion is afforded by the study of the composition of the waters of the Nile, our knowledge of which has been greatly advanced by the recent researches of Dr. C. M. Tidy. In spite of the circumstances that the waters of the Nile must undergo great concentration during its passage of 1,400 miles through regions of exceptional heat and drought, it is found that those waters actually hold in solution little more than one-half the percentage of mineral matter which is present in the river waters of temperate and rainy regions. The chemical disintegration of rocks being so largely due to the action of rain and vegetation, it is not surprising to find that where these agencies are almost entirely absent the rocks exhibit but few signs of chemical change.

The Krakatoa Committee, which is now rather a large one, consisting of thirteen members, has been steadily at work during the year; and the discussion of the very varied and large mass of data has been undertaken by sub-committees, dealing respectively with the following branches:—

Geological—including eruption and earthquake phenomena, and the geological features of the distribution of dust and pumice.

Meteorological (A)—including air waves, sounds, and the geographical distribution of dust and pumice.

Meteorological (B)—including twilight effects, coronal appearances, cloud haze, coloured sun, moon, &c.

Magnetic and electric phenomena.

Tidal waves.

With the exception of the last-named Sub-Committee, viz., that upon Tidal Waves, of which the work has been delayed by the illness of Sir F. Evans, all the reports are now in a forward state, and there seems to be every prospect of the work being concluded in the course of a very few months.

The question of the proper administration of the funds administered on behalf of the Government by the representatives of the Royal Society and of other scientific bodies, who constitute the Government Grant Committee, has frequently been debated with much care by the President and Council, who are held responsible for the final assignment of the grant by the Government.

On the 20th of May last the Council determined, once again, to devote special attention to the subject, and on the 25th June the minutes will inform you that the following resolutions were passed:—

"That in every case of renewed application for a personal grant, after such grant has been received by the applicant in two consecutive years, the application be made not less than three months before it is to be considered, accompanied by a full statement of the case from the applicant, and that before being presented to the Committee, it be referred by the Council to two referees, who shall report to the Council on its merits."

"That the Secretaries be instructed to return to the applicants for aid from the Government Grant such applications as do not in all particulars comply with the conditions laid down in the circular to applicants."

It is very desirable that our intention to enforce the latter resolution strictly should be widely promulgated. I may add that we have considerable reason to complain that too frequently those who have obtained grants through the Committee make no report of the work done to the Society, but leave information on that head to reach us as it may through the publications in which the results obtained by the grantees are made known.

Nineteen large royal quarto volumes of the Official Reports on the Scientific Results of the "Challenger" Expedition have now been issued from the press. These contain thirty-seven zoological, three botanical, and eight physical and chemical reports, together with the narrative of the voyage, which contains the general scientific results of the expedition. Six more volumes are now passing through the press, a considerable part of each being already printed off. The work connected with the remaining memoirs is in a forward state. The whole of the investigations and the manuscript will be completed during the next financial year, and in the course of the year 1887 the whole of

the Reports will be published, and the work connected with the expedition brought to a close.

In the Treasurer's address last year the Society was fully informed of the action taken by the President and Council in the matter of the position of this country with respect to the international "Bureau des Poids et Mesures." I am happy to be able to report to the Society that, last December, we received a letter from the Treasury, stating that my Lords had asked the Secretary of State to instruct the British Ambassador at Paris to make known to the Comité International des Poids et Mesures that Her Majesty's Government were willing to join the Convention on the terms described in our Secretary's letter of the 18th August, 1884, and that the proposal had been accepted.

Your President is, *ex officio*, Chairman of the Board of Visitors of the Royal Observatory. As such, it was my duty to preside at a recent meeting of that body, when my colleagues agreed to recommend the adoption of a day, commencing at midnight, in all observatories and in the Nautical Almanac, from and after the commencement of the year 1891.

Much to my regret, I have been unable to take part in the work of the City and Guilds Institute during the past year; but I have reason to know that considerable progress has been made towards the attainment of its object—the advancement of technical education in London and in the provinces. The Finsbury Technical College is fulfilling its purpose in the most satisfactory manner, and its day and evening classes are so numerously attended that an extension of the building is under consideration. About 250 technical classes in different parts of the kingdom are now affiliated to the Institute, and some of them are already developing into efficient technical schools. The assistance which the Institute is enabled to afford to these classes is restricted by want of means; but there can be no doubt that far larger opportunities of obtaining evening instruction in the application of the different branches of science to industry are afforded to the artisans of London now than was the case even four or five years ago.

Large additions have been made to the equipment of the Central Institution at South Kensington. The engineering laboratory and the extensive chemical and physical laboratories are organised, and the systematic instruction of students has commenced. Scholarships of the value of £30 a-year, tenable for three years, have been offered to and accepted by the Governors of a number of public and other schools. These scholarships are to be awarded by the head master (not necessarily on the result of a competitive examination) to any pupil who is competent to pass the entrance examination of the Central Institution.

The City and Guilds Institute is the outcome of the perception of the necessity for technical education, in the interests of industry, by the wealthiest city and the wealthiest guilds in the world; it may, therefore, seem singular that the chief obstacle to the proper development of the important schools which it has founded is poverty. Such, however, I understand to be the case. The Central Institution requires an assured income of at least £15,000 a-year if it is to work properly; but the joint resources of the City and Guilds of London, at present, appear to be able to afford it only a precarious, annually-voted, subsidy of £9,000 a-year—far less, that is to say, than the private income of scores of individual Londoners. In Germany, a similar institution would demand and receive £20,000 a-year as a matter of course; but Englishmen are famous for that which a perplexed Chancellor of the Exchequer (I think it was) once called their "ignorant impatience of taxation," and there is no occasion on which they so readily display that form of impatience as when they are asked for money for education, especially scientific education. I am bound to add, however, that my experience on the Council and Committees of the Institute has left no doubt on my mind that my colleagues have every desire to carry out the work they have commenced thoroughly; and that the money difficulty will disappear along with certain other difficulties which, I am disposed to think, need never have arisen.

Such are the chief matters of business, if I may so call them, which it is proper for me, in my Presidential capacity, to bring before the Society. But it has been not unusual, of late years, for the occupant of the Chair to offer some observations of a wider bearing for the consideration of the Society; and I am the more tempted to trespass upon your patience for this purpose, as it is the last occasion on which I shall be able to use, or abuse, the President's privileges.

So far as my own observations, with respect to some parts of the field of natural knowledge, and common report, with respect to others, enable me to form an opinion, the past year exhibits no slackening in the accelerated speed with which the physical sciences have been growing, alike in extent and in depth, during several decades. We are now so accustomed to this "unhasting but unresting" march of physical investigation; it has become so much a part of the customary course of events, that, with every day, I might almost say with every hour, something should be added to our store of information respecting the constitution of nature, some new insight into the order of the cosmos should be gained, that you would probably listen with incredulity to any account of the year's work which could not be summed up in this commonplace of Presidential addresses.

Nor shall I be charged with innovation if I add that there is no reason to suspect that the future will bring with it any retardation in the advance of science. The adverse influences, which, in the middle ages, arrested the work commenced by the older Greek philosophers, are so much weakened that they no longer offer any serious obstacle to the growth of natural knowledge; while they are powerless to prevent the extension of scientific methods of inquiry and the application of scientific conceptions to all the problems with which the human mind is confronted. If any prophecy is safe of fulfilment, it is that, in the twentieth century, the influence of these methods and conceptions will be incomparably greater than it is now; and that the interpenetration of science with the common affairs of life, which is so marked a feature of our time, will be immeasurably closer. For good or for evil, we have passed into a new epoch of human history—the age of science.

It may seem superfluous that I should adduce evidence in support of propositions which must have so much of the nature of truisms to you who are sharers in the work of science and daily witnesses of the effects of its productive energy. But the proverbial tendency of familiarity to be incompatible with due respect is noticeable even in our appreciation of the most important truths, and our strongest convictions need furbishing up now and then, if they are to retain their proper influence. I certainly cannot accuse myself of ever having consciously entertained a low estimate of the past work or the future progress of science; but, a few months ago, enforced leisure and the attainment of an age when retrospection tends to become a habit, not to say a foible, led me to look at the facts anew; and I must confess that the spectacle of the marvellous development of science, alike in theory and practice, within my own life-time, appeared to me to justify a faith, even more robust than mine, in its future greatness.

For, if I do not greatly err, the greater part of the vast body of knowledge which constitutes the modern sciences of physics, chemistry, biology, and geology has been acquired, and the widest generalisations therefrom have been deduced, within the last sixty years; and furthermore, the majority of those applications to scientific knowledge to practical ends, which have brought about the most striking differences between our present civilisation and that of antiquity, have been made within that period of time.

To begin with the latter point—the practical achievements of science. The first railway for locomotives, which was constructed between Stockton and Darlington, was opened in September, 1825, so that I have the doubtful advantage of about four months' seniority over the ancestral representative of the vast reticulated fetching and carrying organism which now extends its meshes over the civilised world. I confess it fills me with astonishment to think that the time

when no man could travel faster than horses could transport him, when our means of locomotion were no better than those of Achilles or of Ramses Maimun, lies within my memory. The electric telegraph, as a thing for practical use, is far my junior. So are arms of precision, unless the old rifle be regarded as such. Again, the application to hygiene and to the medical and surgical treatment of men and animals, of our knowledge of the phenomena of parasitism, and the very discovery of the true order of these phenomena, is a long way within the compass of my personal knowledge.

It is unnecessary for me to enumerate more than these four of the many rich gifts made by science to mankind during the last sixty years. Arresting the survey here, I would ask if there is any corresponding period in previous history which can take credit for so many momentous applications of scientific knowledge to the wants of mankind? Depreciators of the value of natural knowledge are wont to speak somewhat scornfully of these and such-like benefactions as mere additions to material welfare. I must own to the weakness of believing that material welfare is highly desirable in itself, and I have yet to meet with the man who prefers material illfare. But even if this should be, as some may say, painful evidence of the materialistic tendencies incidental to scientific pursuits, it is surely possible, without much ingenuity, or any prejudice in favour of one or other view of the mutual relations of material and spiritual phenomena, to show that each of these four applications of science has exerted a prodigious influence on the moral, social, and political relations of mankind, and that such influence can only increase as time goes on.

If the senseless antipathies, born of isolation, which formerly converted neighbours, whether they belonged to adjacent families or to adjacent nations, into natural enemies, are dying away, improved means of communication deserve the chief credit of the change; if war becomes less frequent, it will be chiefly because its horrors are being intensified beyond bearing by the close interdependence and community of interest thus established between nations, no less than by the improvement of the means of destruction by scientific invention. Arms of precision have taken the mastery of the world out of the hands of brute force, and given it into those of industry and intelligence. If railways and electric telegraphs have rendered it unnecessary that modern empires should fall to pieces by their own weight as ancient empires did, arms of precision have provided against the possibility of their being swept away by barbarous invasions. Health means not merely wealth, not merely bodily welfare, but intellectual and moral soundness; and I doubt if, since the time of the father of medicine, any discovery has contributed so much to the promotion of health and the cure of disease as that of the part played

by fungoid parasites in the animal economy, and that of the means of checking them, even though, as yet, unfortunately, it be only in a few cases.

But though these practical results of scientific work, during only two generations, are calculated to impress the imagination, the Fellows of this Society know well enough that they are of vastly less real importance than the additions which have been made to fact and theory and serviceable hypothesis in the region of pure science. But it is exactly in these respects that the record of the past half century is so exceptionally brilliant. It is sometimes said that our time is a day of small things—in science it has been a day of the greatest things, for, within this time, falls the establishment, on a safe basis, of the greatest of all the generalisations of science, the doctrines of the Conservation of Energy and of Evolution.

As for work of less wide scope, I speak in the hearing of those who can correct me if I am wrong, when I say that the larger moiety of our present knowledge of light, heat, electricity, and magnetism, has been acquired within the time to which I refer; and that our present chemistry has been in great part created, while the whole science has been remodelled from foundation to roof. It may be natural that progress should appear most striking to me among those sciences to which my own attention has been directed, but I do not think this will wholly account for the apparent advance "by leaps and bounds" of the biological sciences within my recollection. The cell theory was the latest novelty when I began to work with the microscope, and I have watched the building of the whole vast fabric of histology; I can say almost as much of embryology, since Von Baer's great work was published in 1828. Our knowledge of the morphology of the lower animals and plants, and a great deal of that of the higher forms, has very largely been obtained in my time; while physiology has been put upon a totally new foundation and, as it were reconstructed, by the thorough application of the experimental method to the study of the phenomena of life, and by the accurate determination of the purely physical and chemical components of these phenomena. The exact nature of the processes of sexual and nonsexual reproduction has been brought to light. Our knowledge of geographical and geological distribution, and of the extinct forms of life, has been increased a hundredfold. As for the progress of geological science, what more need be said than that the first volume of Lyell's "Principles" bears the date of 1830?

This brief enumeration of the salient achievements of science in the course of the last sixty years is sufficient not only to justify what I have said respecting their absolute value, but to show how much it excels, both in quantity and quality, the work produced in any corresponding period since the revival of science. It suggests,

as I have said, that science is advancing and will continue to advance with accelerated velocity.

It seems to me, in fact, not only that this is so, but that there are obvious reasons why it must be so. In the first place, the interdependence of all the phenomena of nature is such that a seemingly unimportant discovery in one field of investigation may react in the most wonderful manner upon those which are most widely remote from it. The investments of science bear compound interest. Who could have imagined that a curious inquiry into the relations of electricity with magnetism would lead to the construction of the most delicate instruments for investigating the phenomena of heat; to means of measuring not only the smallest intervals of time, but the greatest depths of the ocean; to methods of exploring some of the most hidden secrets of life? What an enormous revolution would be made in biology, if physics or chemistry could supply the physiologist with a means of making out the molecular structure of living tissues comparable to that which the spectroscope affords to the inquirer into the nature of the heavenly bodies. At the present moment the constituents of our own bodies are more remote from our ken than those of Sirius, in this respect. In the next place, the vast practical importance of the applications of scientific knowledge has created a growing demand for technical education based upon science. If this is to be effective, it means the extension of scientific teaching to all classes of the community, and the encouragement and assistance of those who are fit for the work of scientific investigation to adopt that calling. Lastly, the attraction of the purely intellectual aspects of science and the rapid growth of a sense of the necessity of some knowledge of the phenomena of nature, and some discipline in scientific methods of inquiry, to every one who aspires to take part in, or even to understand, the tendencies of modern thought, have conferred a new status upon science in the seats of learning, no less than in public estimation.

Once more reverting to reminiscence, the present state of scientific education surely presents a marvellous and a most satisfactory contrast to the time, well within my memory, when no systematic practical instruction in any branch of experimental or observational science, except anatomy, was to be had in this country; and when there was no such thing as a physical, chemical, biological, or geological laboratory open to the students of any University,* or to the pupils of any school, in the three kingdoms. Nor was there any University which recognised science as a faculty, nor a school, public

* This statement has been challenged, so far as chemistry is concerned, on behalf of the University of Edinburgh; but Sir Lyon Playfair informs me that, at the time to which I refer, the practical instruction did not go beyond "mere testing exercises for medical students." (T.H.H. Dec. 10, 1885.)

or private, in which scientific instruction was represented by much more than the occasional visit of a vagrant orrery.

At the present moment, any one who desires to obtain a thorough scientific training has a choice among a dozen institutions; and elementary scientific instruction is, so to speak, brought to the doors of the poorer classes. If the rich are debarred from like advantages it is their own affair; but even the most careful public school education does not now wholly exclude the knowledge that there is such a thing as science from the mind of a young English gentleman. If science is not allowed a fair share of the children's bread, it is at any rate permitted to pick up the crumbs which fall from the time-table, and that is a great deal more than I once hoped to see in my lifetime.

I have followed precedent in leading you to the point at which it might be fair, as it certainly would be customary, to end by congratulating you, as Fellows of the Royal Society, on the past progress and the future prospects of the work which, for two centuries, it has been the aim of the Society to forward. But it will perhaps be more profitable to consider that which remains to be done for the advancement of science, than to "rest and be thankful" in the contemplation of that which has been done.

In all human affairs the irony of fate plays a part, and in the midst of our greatest satisfactions, "surgit amari aliquid." I should have been disposed to account for the particular drop of bitterness to which I am about to refer, by the sexagenarian state of mind, were it not that I find the same complaint in the mouths of the young and vigorous. Of late years, it has struck me, with constantly increasing force, that those who have toiled for the advancement of science are in a fair way of being overwhelmed by the realisation of their wishes. We are in the case of Tarpeia, who opened the gates of the Roman citadel to the Sabines, and was crushed under the weight of the reward bestowed upon her. It has become impossible for any man to keep pace with the progress of the whole of any important branch of science. If he were to attempt to do so his mental faculties would be crushed by the multitude of journals and of voluminous monographs which a too fertile press casts upon him. This was not the case in my young days. A diligent reader might then keep fairly informed of all that was going on, without robbing himself of leisure for original work, and without demoralising his faculties by the accumulation of unassimilated information. It looks as if the scientific, like other revolutions, meant to devour its own children; as if the growth of science tended to overwhelm its votaries; as if the man of science of the future were condemned to diminish into a narrower and narrower specialist, as time goes on.

I am happy to say that I do not think any such catastrophe a

necessary consequence of the growth of science; but I do think it is a tendency to be feared, and an evil to be most carefully provided against. The man who works away at one corner of nature, shutting his eyes to all the rest, diminishes his chances of seeing what is to be seen in that corner; for, as I need hardly remind my present hearers, that which the investigator perceives depends much more on that which lies behind his sense organs than on the object in front of them.

It appears to me that the only defence against this tendency to the degeneration of scientific workers, lies in the organisation and extension of scientific education, in such a manner as to secure breadth of culture without superficiality; and, on the other hand, depth and precision of knowledge, without narrowness.

I think it is quite possible to meet these requirements. There is no reason, in the nature of things, why the student who is destined for a scientific career should not, in the first place, go through a course of instruction such as would ensure him a real, that is to say, a practical acquaintance with the elements of each of the great divisions of mathematical and physical science; nor why this instruction in what (if I may borrow a phrase from medicine) I may call the institutes of science, should not be followed up by more special instruction, covering the whole field of that particular division in which the student eventually proposes to become a specialist. I say not only that there is no reason why this should not be done, but, on the ground of practical experience, I venture to add that there is no difficulty in doing it. Some thirty years ago, my colleagues and I framed a scheme of instruction on the lines just indicated, for the students of the institution which has grown into what is now known as the Normal School of Science and Royal School of Mines. We have found no obstacles in the way of carrying the scheme into practice except such as arise, partly, from the limitations of time forced upon us from without; and, partly, from the extremely defective character of ordinary education. With respect to the first difficulty, we ought, in my judgment, to bestow at least four, or better, five, years on the work which has, at present, to be got through in three. And, as regards the second difficulty, we are hampered not only by the ignorance of even the rudiments of physical science, on the part of the students who come to us from ordinary schools, and by their very poor mathematical acquirements, but by the miserable character of the so-called literary training which they have undergone.

Nothing would help the man of science of the future to rise to the level of his great enterprise more effectually than certain modifications, on the one hand, of primary and secondary school education, and, on the other, of the conditions which are attached by the Universities to the attainment of their degrees and their rewards

As I ventured to remark some years ago, we want a most favoured nation clause inserted in our treaty with educators. We have a right to claim that science shall be put upon the same footing as any other great subject of instruction, that it shall have an equal share in the schools, an equal share in the recognised qualification for degrees, and in University honours and rewards. It must be recognised that science, as intellectual discipline, is at least as valuable, and, as knowledge, is at least as important, as literature, and that the scientific student must no longer be handicapped by a linguistic (I will not call it literary) burden, the equivalent of which is not imposed upon his classical compeer.

Let me repeat that I say this, not as a depreciator of literature, but in the interests of literature. The reason why our young people are so often scandalously and lamentably deficient in literary knowledge, and still more in the feeling and the desire for literary excellence, lies in the fact that they have been withheld from a true literary training by the pretence of it, which too often passes under the name of classical instruction. Nothing is of more importance to the man of science than that he should appreciate the value of style, and the literary work of the school would be of infinite value to him if it taught him this one thing. But I do not believe that this is to be done by what is called forming oneself on classical models, or that the advice to give one's days and nights to the study of any great writer, is of much value. "Le style est l'homme même," as a man of science who was a master of style has profoundly said; and aping somebody else does not help one to express oneself. A good style is the vivid expression of clear thinking, and it can be attained only by those who will take infinite pains, in the first place, to purge their own minds of ignorance and half knowledge, and, in the second, to clothe their thoughts in the words which will most fitly convey them to the minds of others. I can conceive no greater help to our scientific students than that they should bring to their work the habit of mind which is implied in the power to write their own language in a good style. But this is exactly what our present so-called literary education so often fails to confer, even on those who have enjoyed its fullest advantages; while the ordinary schoolboy has rarely been even made aware that its attainment is a thing to be desired.

I venture to lay these last observations before you, because we have heard a good deal lately of schemes for the remodelling of the University of London, which has done so much, through its Faculties of Science and Medicine, to promote scientific instruction. As a member of the Senate of the University I am necessarily greatly interested in such projects, and I greatly regret that I have been unable to take part in the recent action concerning them. This is not the time or the place for the discussion of any of these proposals, but many

of my hearers must be as warmly interested in them as I am myself and it may not be out of place to submit two questions for their serious consideration.

In the interests of science, will any change be satisfactory which does not lighten the linguistic burden at present imposed on students of science and of medicine by the matriculation examination?

And again, in the interests of science, will any change be satisfactory which does not convert the examining university into a teaching university? And, by that last term, I do not mean a mere co-operative society of teacher-examiners, but a corporation which shall embrace a professoriate charged with the exposition and the advancement of the higher forms of knowledge in all its branches.

The future both of pure science and of medicine in this country is, I think, greatly interested in the answer which Fellows of this Society, after due meditation, may be disposed to give to these questions.

I have to announce an unusually large number of changes in the staff of the Society.

Last December we regretted to receive the resignation of Mr. Walter White, so long our Assistant Secretary, whose faithful and efficient services, continued for more than forty years, are well known to all the Fellows of the Society. The minutes of the Council record our appreciation of Mr. White's services, and our endeavour to give as substantial a form as possible to our hearty recognition of his deserts. The vacancy thus caused has been filled up by the appointment of Mr. Herbert Rix, whose work since he has held the office of clerk has been such as to justify the confidence of the officers, not only that the functions hitherto discharged by the Assistant Secretary will be as well performed as heretofore; but that, if the interests of the Society should demand it, we may throw still more important duties upon him. I receive the most favourable reports of the efficiency of Mr. James who has been appointed to the office of clerk in place of Mr. Rix.

Notwithstanding my release from all serious work, my health remained so very indifferent for some months after my return to England, that I felt it my duty to the Society to bring the question of my resignation of the Presidency, on the present Anniversary, before the Council which met on the 20th of May. My colleagues were kind enough to wish that my final decision should be deferred, and I need hardly say how willing I should have been to retain my honourable office, if I could have done so with due regard to the interests of the Society, and perhaps I may add, of self-preservation.

I am happy to say that I have good reason to believe that, with prolonged rest—by which I do not mean idleness, but release from distraction and complete freedom from those lethal agencies which

are commonly known as the pleasures of society—I may yet regain so much strength as is compatible with advancing years. But in order to do so, I must, for a long time yet, be content to lead a more or less anchoritic life. Now it is not fitting that your President should be a hermit, and it becomes me, who have received so much kindness and consideration from the Society, to be particularly careful that no sense of personal gratification should delude me into holding the office of its representative one moment after reason and conscience have pointed out my incapacity to discharge the serious duties which devolve upon the President, with some approach to efficiency.

I beg leave, therefore, with much gratitude for the crowning honour of my life which you have conferred upon me, to be permitted to vacate the chair of the Society as soon as the business of this meeting is at an end.

As I am of opinion that it is very undesirable that the President should even seem to wish to exert any influence, direct or indirect, on the action of the Fellows assembled in General Meeting, I am silent respecting the proposals embodied in the new list of the officers of the Society which my colleagues and I have unanimously agreed to submit for your consideration.

The President then proceeded to the presentation of the Medals:—

The Copley Medal is awarded to Professor August Kekulé of Bonn, whose researches in organic chemistry, extended over the last five-and-thirty years, have been fruitful of results of high importance in chemical science. The great work of Professor Kekulé's life, that which has raised him to the highest rank among the investigators of the day, is his general theory of the constitution of carbon compounds, in which the now universally accepted conception of the constitution of those compounds was first clearly and definitely stated.

A development of the fundamental theory led Kekulé to the discovery of the constitution of an exceedingly numerous and very complex class of compounds, which he has named the aromatic compounds, and his theory of the constitution of the aromatic compounds has suggested and guided innumerable investigations. The marvellous success obtained by many of his followers and pupils in building up artificially complex substances which had defied the efforts of all previous investigators, affords tangible evidence that Kekulé's labours have given us a deeper insight into the order of nature.

One of the Royal Medals is awarded to Professor Hughes, F.R.S., for a series of experimental investigations in electricity and magnetism, which are remarkable alike for ingenuity of contrivance, for the simplicity of the apparatus employed, for the delicacy of the indications afforded, and for the wide applicability of the instruments

invented to researches other than those for which they were originally designed.

The microphone, the induction balance, and the sonometer, are instruments by which inconceivably minute electrical and magnetic disturbances not only make themselves loudly audible, but may be definitely measured; and their application has opened up new lines of inquiry.

The other Royal Medal is awarded to Professor E. Ray Lankester, F.R.S., for his labours, now extending over more than twenty years, in the field of animal morphology (especially invertebrate anatomy and embryology) and of palæontology.

Professor Lankester has been active in many directions, and has everywhere left his mark, not only as an energetic teacher and accurate worker and a philosophical thinker; but as one who, in times when the example is more than ever valuable, has always been careful to remember that speculation should be the servant and not the master of the biologist.

The Davy Medal is awarded to Professor Stas of Brussels.

Professor Stas' great research, for which it is proposed that the Davy Medal be awarded to him, is that on atomic weights. There are probably no researches in chemistry, the results of which appeal so little to the imagination, and which are so little applauded as those on atomic weights, yet for difficulty and importance they are hardly surpassed by any. The determination of these fundamental constants of chemistry has engaged the attention of many of the leading chemists, and, before the time of M. Stas' experiments, an immense amount of careful labour had been bestowed on finding methods for the more accurate and complete purification of the compounds employed for the purpose.

The indefatigable and conscientious care which M. Stas has devoted to the re-determination of a certain number of the most important atomic weights, and the marvellous skill with which he has overcome the various difficulties which successively presented themselves, render his memoir on the subject one of the most remarkable and valuable of chemical monographs.

I regret to say that the state of M. Stas' health has not permitted him to be with us to-day, but the representative of his Sovereign, the King of the Belgians, in this country, has kindly consented to receive the medal for him.

M. le Baron Solvyns, I request your Excellency to be so good as to receive the medal awarded to M. Stas; and to assure him of the pleasure which it gives the Royal Society to show their sense of his high merits, by asking his acceptance of this memorial of his illustrious predecessor, Humphry Davy.

The Statutes relating to the election of Council and Officers were then read, and Dr. Gilbert and Mr. Kempe having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.

Professor Robert B. Clifton, M.A.; Professor James Dewar, M.A.; Professor William Henry Flower, LL.D.; Archibald Geikie, LL.D.; Sir Joseph D. Hooker, K.C.S.I.; Professor Thomas Henry Huxley, LL.D.; Admiral Sir A. Cooper Key, G.C.B.; J. Norman Lockyer, F.R.A.S.; Professor Henry N. Moseley, M.A.; Professor Bartholomew Price, M.A.; Professor Pritchard, F.R.A.S.; William James Russell, Ph.D.; Professor J. S. Burdon Sanderson, LL.D.; Professor Arthur Schuster, Ph.D.; Lieutenant-General R. Strachey, R.E., C.S.I.; General James Thomas Walker, C.B.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Dec. 1, 1884 ..	5	46	202	189	77	519
Since Elected ..		+ 2	+ 3	+ 1	+ 12	+ 18
Since Re-admitted				+ 1		+ 1
Since Deceased ..		− 3	− 9	− 9	− 2	− 23
Nov. 30, 1885 ..	5	45	196	182	87	515

Statement of Receipts and Expenditure from November 15, 1884, to November 14, 1885.

		£	s.	d.
Balance at bank, 17th November, 1884	£691 6 4	714	6	1
,, on hand	22 19 9			
Annual Contributions, 190 at £4	760 0 0	1,024	0	0
,, 88 at £3	264 0 0			
Admission Fees		10	0	0
Compositions		180	0	0
Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions		208	0	0
Rents:	£ s. d.			
Fee Farm Rent, Lewes	18 12 11	120	5	1
Mablethorpe Estate	101 12 2			
Ground Rents		675	6	6
Dividends (exclusive of Trust Funds)		1,612	13	5
,, on Jodrell Fund		150	12	3
Interest on Mortgage Loan		582	5	10
Sale of Transactions and Proceedings		119	16	11
Eclipse Expedition (leaving still due £52 15*s.* 3*d.*)		2	15	10
Donation by J. S. Budgett, Esq., for engraving		100	0	0
Matthey and Co., for Limail		20	7	6
Annual Contributions remitted in excess		0	1	6
		£5,520	10	11

	£ s. d.	£	s.	d.
Salaries, Wages, and Pension		1,236	2	0
Catalogue of Scientific Papers		164	12	8
Books for the Library		263	3	11
Printing Transactions, Part II, 1884, and Separate Copies to Authors and Publisher	180 5 0	1,395	18	9
Ditto Proceedings, Nos. 233–237	383 19 10			
Ditto Miscellaneous	98 1 6			
Paper for Transactions and Proceedings	191 19 0			
Binding ditto	69 3 11			
Engraving and Lithography	472 9 6			
Soirée and Reception Expenses		84	0	5
Coal, Lighting, &c.	90 12 8	746	16	5
Office Expenses	43 3 6			
House Expenses	350 10 8			
Tea Expenses	21 16 4			
Fire Insurance	75 2 9			
Taxes	40 12 6			
Advertising	8 13 0			
Postage, Parcels, and Petty Charges	41 11 4			
Miscellaneous Expenses	74 13 8			
Law Charges		25	1	4
Mablethorpe Estate, rent returned		11	0	0
		£3,926	15	6

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	422	0	4			
Rumford Fund "	67	10	4			
Wintringham Fund "	34	17	6			
Bakerian and Copley Medal Fund Dividends	58	4	3	613	16	3
Davy Medal Fund Dividends	28	7	4			
Croonian Lecture Fund Dividend	2	16	6			
				£6,134	7	2

	£	s.	d.	£	s.	d.
Donation Fund	733	14	2			
Davy Medal Fund	32	6	0			
Rumford do. do.	136	1	0	992	3	5
Wintringham Fund	35	5	0			
Copley Medal Fund	54	17	3			
Balances on hand, Catalogue Account				17	9	7
" " Petty Cash				14	17	5
Balance at Bankers				1,183	1	3
				£6,134	7	2

JOHN EVANS,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £110 per annum.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £52 per annum.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £491 12*s*. 10*d*. in 1884–85).

£21,000 { £14,952 12*s*. 3*d*. Reduced 3 per Cent. Annuities.
£6,047 7*s*. 9*d*. " " " Handley Fund.

£15,000 Mortgage Loan, 4 per Cent.

£19,314 0*s*. 2*d*. Consolidated Bank Annuities, being £15,861 19*s*. 1*d*., namely:—

	£	*s*.	*d*.
Donation Fund	6,339	0	1
Rumford Fund	2,322	19	0
Wintringham Fund	1,200	0	0
Gassiot Trust	200	0	0
Sir J. Copley Fund	1,666	13	4
General Purposes	4,133	6	8

and £3,452 1*s*. 1*d*. in Chancery, arising from sale of the Coleman Street Estate.

£403 9*s*. 8*d*. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.

£11,337 17*s*. 3*d*. New Threes { £6,155 2*s*. 5*d*. Scientific Relief Fund.
5,182 14*s*. 10*d*. Jodrell Fund.

£667 5s. 6d. India Fours.

£600 Midland 4 °/o Debenture Stock.—Keck Bequest.

£660 Madras Railway Guaranteed 5 per Cent. Stock.—Davy Medal Fund.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£1,396 Great Northern Railway 4 per Cent. Debentures—The Trevelyan Bequest.

£2,200 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Debenture Stock.—Fee Reduction Fund.

Two Hundred Shares in the Whitworth Land Company, Limited.—Fee Reduction Fund.

£5,000 Madras Railway Guaranteed 5 °/o Stock.

£5,000 North Eastern Railway 4 °/o Stock.

£5,000 London and North Western Consolidated 4 °/o Preference Stock.

£1,400 Great Northern 4 °/o Debenture Stock.—Scientific Relief Fund.

£2,000 South Eastern Railway 4 °/o Debenture Stock.—Darwin Memorial Fund.

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct; and we find that the Balance at the Bankers is £1,183 1s. 3d.

T. H. HUXLEY, *President*.
G. G. STOKES.
M. FOSTER.
HUGO MÜLLER.
R. ETHERIDGE.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct; and we find that the Balance at the Bankers is £1,183 1s. 3d.

J. T. BOILEAU.
JOHN RAE.
W. GRYLLS ADAMS.
W. HUGGINS.

Trust Funds. 1885.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,155	2	5
Great Northern 4 per cent. Debenture Stock	1,400	0	0
Uninvested	33	11	3
	£7,588	13	8

Dr. / *Cr.*

Dr.	£	s.	d.	Cr.		£	s.	d.
To Balance	9	5	10	By Grants		275	0	0
" Dividends	218	12	11	" Bought £400 Great Northern 4 °/o Debenture Stock		466	8	9
" Bentham Bequest	500	0	0	" Balance uninvested	£33 11 3	93	17	6
" Sale of £100 Metropolitan 3½ °/o	107	7	6	" " in hand	60 6 3			
	£835	6	3			£835	6	3

Donation Fund.

£6,339 0*s.* 1*d.* Consols.
The Trevelyan Bequest.
£1,396 Great Northern Railway 4 per Cent. Debenture Stock.

Dr.	£	s.	d.	Cr.	£	s.	d.
To Balance	738	10	3	By Grants	733	14	2
" Dividends	246	5	4	" Balance	426	16	5
" Transferred from Handley Fund	175	15	0				
	£1,160	10	7		£1,160	10	7

Rumford Fund.

£2,322 19*s*. Consols.

	£	s.	d.		£	s.	d.
To Balance	136	1	0	By Medals, &c.	136	1	0
,, Dividends, 1885	67	10	4	,, Balance	67	10	4
	£203	11	4		£203	11	4

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s*. 4*d*. Consols.
£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	105	0	1	By Gold Medal	4	17	3
,, Dividends, three quarters	9	15	6	,, Prof. Ludwig, Sir J. Copley's Gift	50	0	0
,, Dividend—Sir J. Copley's Fund	48	8	9	,, Balance	108	7	1
	£163	4	4		£163	4	4

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1885	23	6	6	By Payment to Foreign Secretary	23	6	6

Wintringham Fund.

£1,200 Consols.

	£	s.	d.		£	s.	d.
To Balance, 1884	35	5	0	By Payment to Foundling Hospital, 1885	35	5	0
,, Dividends, 1885	34	17	6	,, Balance	34	17	6
	£70	2	6		£70	2	6

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To Balance, 1884	2	18	9	By Balance	5	15	3
„ One-fifth of Rent of Estate at Lambeth Hill, receivable from the College of Physicians	2	16	6				
	£5	15	3		£5	15	3

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	78	4	9	By Gold Medals	32	6	0
„ Dividends	28	7	4	„ Balance	74	6	1
	£106	12	1		£106	12	1

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£200 3 per Cent. Consols.

	£	s.	d.		£	s.	d.
To Balance	291	19	8	By Payments to Kew Committee	491	8	10
„ Dividends	497	5	1	„ Italian Irrigation Bond (£100) and expenses	130	2	6
				„ Balance	167	13	5
	£789	4	9		£789	4	9

Handley Fund.

£6,047 7*s.* 9*d.* Reduced.

	£	s.	d.		£	s.	d.
Dividends, 1885	175	15	0	By transferred to Donation Fund	175	15	0

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1885	150	12	3	By transferred to Royal Society General Account	150	12	3

Fee Reduction Fund.

£2,200 Metropolitan Consols 3½ per Cent.

£7,000 London and North Western Railway 4 per Cent. Debentures.

Two Hundred Shares in the Whitworth Land Company, Limited.

	£	s.	d.		£	s.	d.
To Balance (1884)	229	19	0	By transferred to Royal Society General Account (1885)	208	0	0
,, Dividends (1885)	558	11	6	,, Ditto ditto ditto	4	0	0
				,, Balance	576	10	6
	£788	10	6		£788	10	6

Darwin Memorial Fund.

£2,000 South Eastern Railway 4 per Cent. Debenture Stock.

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XXXVII, p. 457).

1884–85.

	£
Dr. D. Gill, for reducing and discussing several series of observations of the minor planets Victoria and Sappho, for the purpose of determining the Solar parallax..................	100
Council of the Royal Astronomical Society (per E. B. Knobel), for comparison of Greenwich Lunar Observations, 1847–61, with Hansen's "Tables de la Lune"	320
Dr. D. Gill, for assistance and chemicals to obtain (1) a Series of daily photographs of the Solar Corona; (2) Photographic Star-maps of the Southern Heavens................	300
Prof. J. A. Ewing, towards the construction and establishment of Seismometers at the Observatory on Ben Nevis	200
Prof. W. N. Hartley, for Materials, Apparatus, and Assistance for continuing his investigations in (1) Spectrum Photography, in relation to New Methods of Quantitative Chemical Analysis; (2) On the relation between the Absorption Spectra and the Molecular Constitution of Organic Compounds	100
H. B. Guppy, for the exploration of the Interior of the Islands of Guadalcanar and St. Christoval (Solomon's Group)..	150
W. H. Caldwell, for further aid in studying the development of Ceratodus, Monotremata, and Marsupials in Australia	200
Dr. S. J. Hickson, towards the expense of a voyage to the Malay Archipelago with a view to certain Anatomical and Embryological Researches.............................	200
Dr. H. Hicks, for the expense of Exploring two Bone Caverns, situated in the Carboniferous Limestone on the east side of the Vale of Clwyd, North Wales	50
J. S. Gardner, for continuing Investigations into the Tertiary Inter-basaltic Floras of Ireland, Scotland, and Iceland	75
F. R. Japp, for a study of the Reactions of Quinones, Diketones, and allied compounds...........................	75
Henry Robinson, for continuation of Researches on the Atomic Weights of the Metals of the Cerium and Yttrium Groups..	50
Carried forward................	£1,820

Brought forward................	£1,820
N. Collie, for completing a Research on the Salts of Tetrethyl Phosphonium	20
H. Tomlinson, for continuation of his Researches on the Influence of Stress and Strain on the Physical Properties of Matter	50
H. R. Mill, for a Research on the Chemistry of Estuary Water	200
Wilson L. Fox (R. Cornwall Polytechnic Society), for the purchase of a Dip Circle and a Unifilar (£80), and furnishing the Unifilar with an Altazimuth (£20) for the Falmouth Observatory..	100
Prof. P. G. Tait and A. Buchan, for an Investigation into the best form of Apparatus for the observation of the actual Temperature of the Air apart from the effects of the Sun's Rays, and other circumstances, on the Thermometers used....	50
Dr. Gore, for expense (including assistance) of an Experimental Investigation of the subject of "Transfer-resistance" in Electrolytic and Voltaic Cells	150
J. Kerr, for continuation of Optical and Electro-optical Researches	50
R. Milne Murray, for continuation of his Research on the Innervation of the Uterus..................................	50
Rev. A. E. Eaton, to defray the cost of Illustrations of a Revisional Monograph of Terrestrial Isopod Crustacea	100
Dr. L. C. Wooldridge, for the expense of Researches on the Physiology and Pathology of the Blood....................	50
H. T. Stainton, in aid of the Publication Fund of the Zoological Record Association..........................	150
Victor Horsley, for a Research on the Functions of the Thyroid Body	50
J. J. Harris Teale, for aid in the Collection of Specimens and Preparation of Sections to illustrate a work on Petrography..	60
E. A. Schäfer, for additional assistance in Researches into the Physiology of the Nervous System	50
R. Kidston, for Investigations in the Distribution of the Carboniferous Fossil Plants	40
G. C. Bourne, towards the expense of an Expedition to the Chagos Islands, with a view to study their Fauna and Flora..	100
J. T. Cunningham, for continuation of a Research on the Development of Marine Teleostean Fishes, and of *Myxine glutinosa*	100
Carried forward................	£3,090

Brought forward................	£3,090
Spencer U. Pickering, for an Investigation on Molecular Compounds..	100
O. Masson and L. Dobbin, for a Research on the Action of the Halogens on the Salts of Organic Bases	25
W. R. Dunstan, for an Investigation of the Chemical and Physiological Properties of the Glucoside (Loganin) which exists in Strychnos Nux Vomica............................	15
C. F. Cross, E. J. Bevan, and C. S. S. Webster, for aid in re-investigating the substance Phenose and the Chemistry of Liquefaction	125
Percy F. Frankland, for an Assistant in a Research on the Action of various Chemical and Mechanical Agencies upon the Vitality and Distribution of Micro-organisms in Air and Water	50
W. E. Adeney, for determining the Wave-lengths and mapping the lines of the Ultra-violet Spark Spectra of Chromium and Manganese, and those lines of iron more refrangible than wave-length 2146	50
T. Rupert Jones, for continuing Examination of the Fossil Ostracoda	100
Prof. W. K. Parker, for continuation of Researches on the Morphology of the Vertebrata..........................	300
Walter Gardiner, for Investigations on the various Phenomena in connexion with Insectivorous Plants	100
H. B. Baker, for Investigating whether the Presence of a third body is necessary for the Combustion of Elementary Bodies in Oxygen	100
	£4,055

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Grant from Treasury	4,000	0	0	By Balance, Dec. 1, 1884	338	19	6
To Repayments	150	0	0	By Appropriations, as above............	4,055	0	0
To Interest on Deposit.........	17	2	5	Printing, Postage, Advertising, and other Administrative Expenses	74	15	4
To Balance on hand, Nov. 30, 1885	301	12	5				
	£4,468	14	10		£4,868	14	10

Account of Grants from the Donation Fund in 1884–85.

	£	s.	d.
Prof J. J. Thomson, for Apparatus to make quantitative experiments in the decomposition of steam under various pressures by the silent electrical discharge	25	0	0
Dr. Gaskell and Mr. Gadow, to further assist them in their anatomical and physiological Researches on Reptiles	30	0	0
Dr. Brunton, for continuation of his Researches on the connexion between Chemical Constitution and Physiological Action	50	0	0
A Committee of the Royal Society, for further aid in photographing the Corona of the Sun and carrying out other physical observations at high elevation.............	36	14	2
Prof. Lankester, in aid of an investigation into the Freshwater Fauna of England	50	0	0
Mr. Harvey Gibson, for aid in the prosecution of his Researches into the Embryology of *Patella vulgata*	20	0	0
The Marine Biological Association, for the erection of marine laboratories on the coast of Great Britain	250	0	0
Prof. Schäfer, for further aid in his Researches into the Physiology of the Heart, and the Electrical Condition of Secreting Glands	20	0	0
Dr. Sclater, to secure the services of Mr. W. H. Hudson in revising and preparing for publication a work on the Birds of the Argentine Republic	40	0	0
The Krakatoa Committee, for further Expenses in collecting and classifying notices of the Volcanic Outbursts in the Straits of Sunda	50	0	0
Prof. Roy, for aid in investigating in Spain the nature of Cholera	150	0	0
Prof. Judd, for aid in making excavations at Cutties' Hillock Quarry, near Elgin	12	0	0
	£733	14	2

Report of the Kew Committee for the Year ending October 31, 1885.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. Warren de la Rue, *Chairman*.

Captain W. de W. Abney, R.E.
Prof. W. G. Adams.
Capt. Sir F. Evans, K.C.B.
Prof. G. C. Foster.
Mr. F. Galton.
Vice-Adm. Sir G. H. Richards, C.B.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-General W. J. Smythe.
Lieut.-Gen. R. Strachey, C.S.I.
Mr. E. Walker.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches.
7th. Miscellaneous.

I. Magnetic Observations.

The Magnetographs have been in constant operation during the year, and in accordance with the usual practice, determinations of the scale values of all the instruments were made early in January.

As regards magnetic disturbances, no very exceptional variations have been registered. The principal perturbations occurred on the following dates:—November 2-3, 1884; March 15-16, May 13 and 27, and June 25-26, 1885.

In February the Kew 9-inch Unifilar Magnetometer by Jones was

sent to Messrs. Elliott Brothers, London, for the purpose of having Mr. Whipple's arrangement for steadying the Collimator Magnet fitted, and at the same time a rack and pinion adjustment was attached to the small telescope employed in viewing the collimator scale, in order to bring the scale more readily into focus.

The values of the ordinates of the different photographic curves determined in January were as follows:—

Declination: 1 inch $=0^\circ\ 22'\cdot04$. 1 cm. $=0^\circ\ 8'\cdot7$.

Bifilar, January 13, 1885,	for	1 inch δH	$=0\cdot0268$ foot grain unit.
	,,	1 cm. ,,	$=0\cdot0005$ C.G.S. unit.
Balance, January 14, 1885	,,	1 inch δV	$=0\cdot0280$ foot grain unit.
	,,	1 cm. ,,	$=0\cdot0005$ C.G.S. unit.

Information on matters relating to terrestrial magnetism and various data have been supplied to Dr. Wild, Professor Mascart, Dr. Van der Stok, Mr. R. H. Scott, Professor W. G. Adams, Dr. Rijckevorsel, Professor Rücker, and Dr. Atkinson.

The monthly observations with the absolute instruments have been made as usual, and the results are given in the tables forming Appendix I of this Report.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of	Horizontal Intensity........	34
,,	Inclination................	127
,,	Absolute Declination........	53

International Polar Commission.—The magnetic observations made during the year September, 1882, to August, 1883, at Fort Rae, North America, by the expedition under Captain Dawson, R.A., have been fully reduced and prepared for publication, on the plan adopted by the International Polar Commission at their Meeting at Vienna in 1884, by the Observatory staff during extra office hours, and the work is at present passing through the press. The readings of the Kew Magnetographs have also been reduced on the same plan at the cost of the Polar Committee of the Royal Society, and copies forwarded to Dr. H. Wild, President of the Commission. Special scales were constructed for the tabulation of the Kew curves on the C.G.S. system by Mr. Baker, the magnetic observer.

Krakatoa Eruption.—The Krakatoa Committee of the Royal Society having entrusted to the Kew Committee the data which they have collected relating to electrical and magnetical phenomena which occurred about August 27, 1883, the date of the eruption of Krakatoa, the curves of the magnetographs of the Observatories at

Batavia, Colàba, Lisbon, Mauritius, Melbourne, Paris, Stonyhurst, and Zi-ka-wei have been carefully compared with each other, and a report thereon will shortly be submitted to the Committee.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of atmospheric pressure, temperature, and humidity, wind (direction and velocity), bright sunshine, and rain, have been maintained in regular operation throughout the year.

In February the barometer tube was removed for a short time from the barograph, and a carefully divided glass scale substituted in its place, which was then photographed with the view of re-determining the scale value of the instrument, and also of measuring the amount of distortion the curve undergoes by shrinkage of the gelatinized paper during the photographic operations, to which it is subjected, subsequent to its reception of the luminous image.

In September the action of the barograph was observed to be somewhat sluggish, and an examination of the instrument showed an obstruction of the air-vent in the cistern due to an accumulation of dust. This was removed, and there has since been no want of sensitiveness on the part of the barometer.

The standard eye observations for the control of the automatic records have been duly registered during the year, together with the daily observations in connexion with the U.S. Signal Service synchronous system. A summary of these observations is given in Appendix II.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted as usual to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer	1750
,, dry and wet thermometers	3460
,, maximum and minimum thermometers	730
,, radiation thermometers	2825
,, rain gauges	730
Cloud and weather observations	1825
Measurements of barograph curves	8760
,, dry bulb thermograph curves	9490
,, wet bulb thermograph curves	8760
,, wind (direction and velocity)	17520
,, rainfall curves	680
,, sunshine traces	2079

In compliance with a request made by the Meteorological Council to the Committee, Mr. Whipple visited Falmouth in May in order to superintend the removal of the meteorological instruments from the old Observatory to the new building recently erected near that town by the Royal Cornwall Polytechnic Society; he has since inspected the instruments at the Aberdeen, Stonyhurst, and Glasgow Observatories, and the Anemographs at Yarmouth and Sandwick.

Mr. Baker visited the Valencia and Falmouth Observatories for the purpose of inspection during his vacation.

With the sanction of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by "The Times" and "The Torquay Directory." Data have also been supplied to the Council of the Royal Meteorological Society, the editor of "Symons's Monthly Meteorological Magazine," the Secretary of the Institute of Mining Engineers, Messrs. Gwilliam, Rowland, and others. The cost of these abstracts is borne by the recipients.

Electrograph.—The difficulty of maintaining the potential of the charge of this instrument constant, mentioned in last year's Report, having greatly increased, in spite of all measures of precaution which were taken, the Meteorological Council were in July informed of its unsatisfactory condition. They accordingly gave instructions to discontinue its working, and it is intended to draw up a report on the results which may be obtained from the eleven years' curves of variations of atmospheric electricity now available for discussion.

III. Solar Observations.

The sketches of Sun-spots, as seen projected on the photoheliograph screen, have been made on 170 days, in order to continue Schwabe's enumeration, the results being given in Appendix II, Table IV.

Transit Observations.—320 observations of solar and 102 of sidereal transits have been taken, for the purpose of keeping correct local time at the Observatory, and the clocks and chronometers have also been compared daily. The Observatory Chronometer, Breguet 3140, has been cleaned and readjusted.

The following clocks, French, Shelton K. O., and Dent 2011, and the chronometers, Molyneux No. 2125, and Breguet No. 3140, are kept carefully rated as time-keepers at the Observatory.

IV. Experimental Work.

Photo-nephograph.—The experiments with the photo-nephographs having proved satisfactory, and a report to that effect having been presented to the Meteorological Council, it was decided in June to

take frequent pictures for the purpose of determining the rate of motion of clouds. Accordingly the telegraph cable uniting the two stations was buried in the ground (permission having previously been granted by the lessee of the Old Deer Park), and the stands and electrical fittings were made fixtures.

A quantity of photographic plates, prepared in accordance with Captain Abney's formula, were also obtained from a manufacturer, and certain arrangements made in the photographic laboratory of the Observatory for their convenient manipulation. Blank forms for the computation of the cloud positions and motions were also drawn up and printed.

Between July 6th, when the installation of the apparatus was completed, and the date, when the experiments were brought to a close, in accordance with the instructions of the Meteorological Council, 168 cloud negatives were obtained on 23 days, from these 62 approximate determinations of the rate and direction of motion of clouds at heights varying from 3,000 feet to 50,000 feet have been secured.

A detailed report of the work is being drawn up for presentation to the Meteorological Council.

Solar Radiation Thermometers.—The Committee have made a great number of experiments on the construction and exhaustion of the solar radiation thermometers, and the Superintendent is engaged on a report to be communicated to the Royal Society. The general result would indicate that solar radiation as measured by the black bulb thermometer in vacuo has hitherto been considerably underrated.

Baily's Wind Integrator.—This instrument, after working successfully with electrical counters for some time, was simplified by the inventor by the substitution of mechanical counters. These being found to work satisfactorily, Mr. Baily removed the instrument in May for the purpose of exhibiting it at the International Inventions Exhibition.

The spare Beckley Anemograph to which it was attached has been dismounted, and together with the de la Rue recorder (see report for 1879) has, by direction of the Meteorological Council, been forwarded to Mr. Munro to be reconstructed as a Beckley recorder of the original type.

Electrical Anemograph.—The Meteorological Council having granted a sum of money for experiment, and placed at the disposal of Mr. W. Preece, F.R.S., Superintendent of Telegraphs, an old Beckley Anemograph of the 1858 model, that gentleman had it fitted up by Mr. Kempe, of the Chief Engineer's Department, G.P.O., so as to record electrically, and it was erected on the Experimental House of the Observatory. The velocity attachment has worked most satisfactorily for the past six months, neither batteries

nor connexions having needed the slightest attention. The direction gear has, however, occasionally required readjustment of its orientation after strong winds have blown, and is now undergoing alteration.

Hand Anemometers.—A number of these instruments, intended to show the velocity of the wind during a brief period of observation, have had their scale values determined by direct comparison with the Standard Anemograph of the Observatory.

Range-finders.—Facilities have been afforded to Dr. Ristori, F.R.A.S., by the employment of the Cooke apparatus, for the purpose of graduating some new range-finders invented by Mr. Nordenfeldt, and constructed by Mr. Casella, the cost of the experiments being defrayed by the inventor.

V. Verification of Instruments.

The following magnetic instruments have been verified, and their constants determined:—

2 Unifilar Magnetometers and an Inclinometer for Elliott Brothers, London.

1 Unifilar Magnetometer with two Collimating Magnets, and an Inclinometer for the Admiralty, London.

1 small-pattern Fox Circle for the Bureau of Navigation, United States Government, and an ordinary Inclinometer for Dover, Charlton.

3 pairs of Inclinometer Needles have been purchased on commission and verified for Dr. Wild and the Mauritius Observatory.

One Unifilar and an Inclinometer are at present undergoing examination for the Falmouth Observatory.

The total number of other instruments tested in the past year was as follows:—

Barometers, Standard	54
,, Marine and Station	98
Aneroids	104
Total	256

Thermometers, ordinary Meteorological	1825
,, Standard	143
,, Mountain	13
,, Clinical	8238
,, Solar radiation	49
Total	10268

Hydrometers	461
Anemometers	14
Rain Gauges	20
Sextants	130
Index and Horizon Glasses, unmounted	74
Dark Glasses, unmounted	235

Besides these, 38 Deep-sea Thermometers have been tested, 32 of which were subjected, in the hydraulic press, without injury, to pressures exceeding two tons on the square inch. 55 Thermometers have been compared at the freezing-point of mercury, making a total of 10,361 for the year.

Duplicate copies of corrections have been supplied in 43 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	52
„ ordinary meteorological	4
Various	97

3 Standard Thermometers have also been calibrated, and supplied to societies and individuals during the year.

1 Evaporation Gauge, 4 Thermograph Thermometers, 1 Sunshine Recorder, 2 Gauge Barometers for comparing Aneroids, 1 Electrical Anemometer, and 1 Richard Thermograph were also tested.

There are at present in the Observatory undergoing verification, 2 Barometers, 222 Thermometers, 100 Hydrometers, 24 Sextants, and 1 self-recording Aneroid.

Sextant Testing Apparatus.—In consequence of the increasing number of sextants sent to the Observatory for examination it was found desirable to provide a special accommodation in the building for the work. As the room known as the Pendulum room was unoccupied, and the masonry pier fitted up in it as a support for pendulum apparatus was no longer required, it was resolved to convert the apartment into a sextant room, and accordingly the pier was removed, and the Cooke testing apparatus dismounted from the South Hall and re-erected on its site. Four careful redeterminations were then made of the angles between the collimators, and they were found to have been unaffected by the transfer of the apparatus to the new position.

VI. Rating of Watches.

The arrangements for rating watches mentioned in previous Reports have been continued during the year with great success, and

up to the present 367 watches have been tried, of which 39 were submitted by the owners, and 328 by the manufacturers or dealers.

The 302 watches received during the year were entered for testing in the following classes:—

For class A, 254; class B, 38; and class C, 10. Of these 72 failed to gain any certificate; 6 passed in C, 60 in B, 110 in A, and 6 others obtained the highest possible form of certificate, the class A *especially good.*

Owing to numerous requests from manufacturers and others, a system of awarding marks to class A watches, indicating the degree of relative efficiency exhibited during trial, was adopted, being based upon plans already in use in the Geneva and Yale Observatories. In it the number of marks awarded to a watch that only just succeeds in obtaining an A certificate is 0, but to an absolutely perfect watch would be 100, made up as follows:—40 for a complete absence of variation of daily rate, 40 for absolute freedom from change of rate with change of position, and 20 for perfect compensation for effects of alteration of temperature.

As, however, the trials already in use do not comprise a test for the going of travellers' or explorers' watches, experiments are in progress with a view of constructing apparatus to test the behaviour of watches when kept in motion, as in the case of daily wear and travelling, in order to make a special examination on this point for watches submitted for trial by the Royal Geographical Society.

A series of tests for pocket chronographs has also been introduced by special request of the Cyclists' Union.

In Appendix III will be found a table giving the results of trial of the watches which have gained the highest certificates in each class.

The following table will indicate the nature of the trials to which ordinary certificates refer:—

Position of watch during test.	For certificate of Class		
	A.	B.	C.
Vertical, with pendant up	10 days	14 days	8 days
" " " right	5 "	—	—
" " " left	5 "	—	—
Horizontal, with dial up	5 "	14 days	8 days
" " " down	5 "	—	—
" at temp. 85° F.	5 "	1 day	—
" " 35° F.	5 "	1 "	—
Not rated	5 "	1 "	—
Total duration of test	45 days	31 days	16 days

VII. Miscellaneous.

Photographic Paper, &c.—This has been supplied to the Observatories at Alipore, Colàba, Falmouth, Glasgow, Mauritius, Stonyhurst, St. Petersburg, and Toronto, and to the Meteorological Office.

*History of the Observatory.**—A paper giving a history of the Kew Observatory from its earliest foundation down to the present date has been compiled by Mr. R. H. Scott, a member of the Committee, and printed in the "Proc. Roy. Soc.," vol. xxxix, p. 37.

Dowson Gift.—The Committee are indebted to Mr. E. T. Dowson, F.R. Met. Soc., for the presentation of a large collection of weights and measures formed by the late Mr. James Yates, F.R.S., member of the Metric Committee of the British Association, with books and pamphlets bearing on the Metric System. At the request of Mr. H. J. Chaney, Warden of the Standards, a number of these works, copies of which were not to be found in the Library of the Standards Office, were handed over by the Committee to that Department.

The Observatory has also been presented by the Rev. John Rigaud, B.D., Fellow of Magdalen College, Oxford, with a framed sketch portrait of his father, Stephen Peter Rigaud, Esq., M.A., F.R.S., Savilian Professor of Astronomy and Radcliffe Observer, who, in the early part of the present century, during the Oxford vacations, was in the habit of relieving his uncle, the Rev. S. Demainbray, of his charge as "The King's Observer at Kew."

Exhibition.—A number of instruments of interest were exhibited at the Sixth Annual Exhibition of the Royal Meteorological Society, which was devoted to sunshine recorders and actinometry, and held in the rooms of the Institution of Civil Engineers in March last.

International Inventions Exhibition.—The Committee have exhibited in Groups 27 and 29 at this Exhibition articles of which the following is the description as given in the Official Catalogue:—

"Forms and papers illustrating the methods employed at the Kew Observatory, Richmond, in examining, rating, and certifying as to the performance of watches, pocket chronometers and chronographs for the manufacturers and general public."

"(1.) Photo-nephograph or Cloud-height Measuring Apparatus. (2.) Apparatus employed in the examination and testing of sextants, quadrants, theodolites, &c. (3.) Specimens of certificates awarded to instruments, and general information relating thereto."

The Jury Commission has awarded to the Committee a Diploma of Honour for their exhibits.

The Superintendent, with the consent of the Committee, read the

* This paper was based upon a short note on the History of the Observatory, submitted by Mr. McLaughlin, one of the staff, to a local Society in Richmond.

following papers before the Aberdeen Meeting of the British Association:—

"On the Errors of first class Sextants, as determined from the Records of the Verification Department at the Kew Observatory;" and "On the Behaviour of first class Watches whilst undergoing Tests in the Rating Department of the Kew Observatory."

At the request of the Royal Cornwall Polytechnic Society, the Kew Committee have undertaken the purchase and trial of a set of Magnetographs now in course of construction for the Falmouth Observatory, on a new plan, the designs and specification for which have been prepared by Mr. Whipple, as the Royal Society grant was inadequate to provide for instruments of the ordinary Kew pattern.

By the kindness of Captain Rung, of the Meteorological Institute, Copenhagen, the Superintendent has been able to procure two specimens of his apparatus for whirling thermometers. These, with the necessary thermometers, have been forwarded to Dr. Doberck, the Government Astronomer at Hong Kong.

Magnetic Disturbances.—By permission of the Committee, Mr. W. Lant Carpenter has visited the Observatory for the purpose of extracting certain magnetic information from the tabulations, in order to assist Professor Balfour Stewart in his investigations on Terrestrial Magnetism.

Workshop.—The machine tools procured by grants from the Government Grant Fund or the Donation Fund for the use of the Kew Observatory have been kept in thorough order. In consequence of the increased number of clinical thermometers submitted for verification, a new specially constructed Galton testing apparatus has been purchased at a cost of 38*l.*, as well as a duplicate Hall-marking apparatus. Accommodation has been found in the workshop for the assistants engaged in the new department specially devoted to the examination of this class of instruments.

Library.—During the year the Library has received, as presents, the publications of—

26 Scientific Societies and Institutions of Great Britain, and

78 Foreign and Colonial Scientific Societies and Institutions.

House, Grounds, and Footpath.—These have all been kept in order during the year. A step ladder has been set up to give more convenient access to the roof of the Sun-room for the purpose of testing Anemometers. The dome has also been lifted and its fittings readjusted. The necessary external repairs to the building, as well as an examination and cleaning of the drains, have been effected by Her Majesty's Commissioners of Works.

The Committee has addressed a memorial to Her Majesty's Commissioners of Woods and Forests, through the President and Council of the Royal Society, with the object of securing free passage to the

Observatory at all hours through the yard tenanted by the lessee of the park at the entrance gates, and negotiations are in progress on the subject.

The Committee has effected an insurance of the contents of the Observatory and outbuildings against loss by fire in the Liverpool, London, and Globe Fire Insurance Company.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant and Magnetic Observer.
H. McLaughlin, Librarian and Accountant.
E. G. Constable, Solar Observations and Watch Rating.
W. Hugo, J. Foster, T. Gunter, W. Boxall, } Verification Department.
E. Dagwell.
H. A. Widdowson.
F. Oliver.
W. C. Gough.
E. Redding.
M. Baker, Messenger and Care-taker.

The following resignations have taken place during the year:—H. Barton, C. Henley, and A. Nish.

Abstract. The Kew Observatory Receipts and Payments Account from November 1, 1884, to November 7, 1885.

Dr. RECEIPTS.

				£	s.	d.
To Balance from 1883–84				482	7	0
Royal Society (Gassiot Trust)				491	8	10
Meteorological Office				400	0	0
Meteorological Office, for Postages, &c.				12	1	10
Instruments on Commission				242	7	2
Experimental Work for Meteorological Office	36	6	3			
,, ,, Sanitary Institute	6	9	11			
				42	16	2
Sale of Standard Thermometers	5	9	6			
"Times" Diagrams and Copying Registers, &c.	62	2	0			
				67	11	6
Verification Fees, Meteorological Office	90	15	0			
,, ,, Observatories and Institutions	29	6	8			
,, ,, Instrument Makers and others	607	2	11			
				727	4	7
Watch Rating Fees, &c.				185	8	6
Carried forward				£2651	5	7

PAYMENTS. *Cr.*

				£	s.	d.
By Salaries	£1199	4	8			
Extra Payments	23	11	0			
				1222	15	8
Fuel and Gas	53	11	6			
Furniture and Fittings	10	17	0			
Chandlery, &c.	7	15	8			
Painting and Repairs	48	12	9			
Rent and Maintenance of Enclosure and Road	22	5	2			
				143	2	1
Printing, Stationery, and Wood-engraving	36	15	11			
Postages, &c.	12	2	10			
Library	14	10	5			
Messenger and Housekeeper	58	6	0			
Porterage, Insurance, &c.	19	19	0			
				141	14	2
Chemicals and Photographic Paper	28	6	4			
Clinical Thermometer Tester	37	8	9			
Anemograph Sheets	2	4	7			
Repairs and Purchase of Instruments	24	15	9			
Carpenter's Work and Sundries	22	19	4			
				115	14	9
Postages, Porterages, &c., for Meteorological Office				14	9	3
Instruments and Paper purchased on Commission				203	8	4
International Circumpolar Committee				19	8	10
Experimental Account—The Observatory	8	2	8			
,, ,, Meteorological Office	81	17	2			
,, ,, W. Baily, Esq.	3	18	10			
,, ,, Sanitary Institute	4	4	0			
				98	2	8
"Times" Diagrams, Copying, &c.				30	16	6
Exhibitions (Models, Mounts, Porterage, &c.)				25	12	8
Verifications. Extra Payments	78	5	0			
,, Ice, Carbonic-Acid Gas and Tubes	18	11	9			
,, Postages and Porterages	10	5	0			
,, Printing and Stationery	27	8	0			
,, Pantagraph, Packing Cases, &c.	7	0	2			
				141	19	11
Carried forward				£2157	4	10

	£	s.	d.
Brought forward	2651	5	7
	£2651	5	7

	£	s.	d.	£	s.	d.
Brought forward				2157	4	10
Watch Rating.—Extra Payments	43	16	0			
" Advertisements, Ice, Tools, &c.	17	3	1			
" Postages and Porterages	6	16	1			
" Printing and Stationery	3	9	7			
" Carpenter's Work, &c.	5	17	3			
				77	2	0
Balance—Bank of England	331	8	3			
London and County Bank	66	0	5			
Cash in hand	19	10	1			
				416	18	9
				£2651	5	7

November 19, 1885. Examined and compared with the Vouchers, and found correct. (Signed) ROBERT H. SCOTT, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	416	18	9
Meteorological Office, Allowances, Experimental, and Sundries	105	17	3
Verification Fees due, &c.	84	11	11
Watch Rating Fee due	2	19	6
Photographic Paper	13	0	0
Commissions, &c.	20	9	10
Blank Forms	7	2	9
Standard Thermometers	99	12	0
	£750	12	0

LIABILITIES.

	£	s.	d.
To Gas, Fuel, and House Account	8	19	8
Apparatus, Chemicals, &c.	4	6	9
Commissions, &c.	3	2	0
Verification Account	3	15	4
Watch Rating Account	1	2	6
Balance	729	5	9
	£750	12	0

November 19, 1885. (Signed) G. M. WHIPPLE, *Superintendent.*

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* 0^h 1^m $15^s{\cdot}1$ *W., for the year October* 1884 *to September* 1885.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets 101 B and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are 3½ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0{\cdot}00129$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have been made and reduced by Mr. T. W. Baker.

Vibration Observations for Absolute Measure of Horizontal Force.

Table I.

Month.	G. M. T.	Temperature. Fahr.	Time of one Vibration.*	Log *m*X. Mean.	Value of *m*.†
1884.	d. h. m.	°	secs.		
October..........	29 11 25 A.M.	48·1	4·6517	0·30820	0·51988
	30 12 29 P.M.	56·3	4·6542		
	2 17 P.M.	57·9	4·6530	0·30841	0·51976
November........	27 11 44 A.M.	46·7	4·6472		
	3 51 P.M.	49·3	4·6473	0·30906	0·51979
December........	29 11 57 A.M.	39·1	4·6493		
	2 55 P.M.	40·3	4·6458	0·30850	0·51948
1885.					
January..........	29 1 50 P.M.	55·0	4·6509	0·30880	0·51978
	30 12 19 P.M.	51·6	4·6543	0·30798	0·51937
February	26 11 52 A.M.	51·4	4·6503		
	3 6 P.M.	54·0	4·6482	0·30898	0·51970
March...........	23 12 10 P.M.	47·0	4·6494		
	2 10 P.M.	48·0	4·6477	0·30876	0·51965
	24 12 21 P.M.	49·1	4·6496		
	2 3 P.M.	51·6	4·6498	0·30870	0·51970
April............	27 11 38 A.M.	64·7	4·6555		
	2 55 P.M.	71·8	4·6552	0·30875	0·51971
	28 2 19 P.M.	74·2	4·6590	0·30846	0·51976
May....	28 11 12 A.M.	68·8	4·6608		
	2 55 P.M.	75·0	4·6583	0·30822	0·51983
	29 2 9 P.M.	67·5	4·6566	0·30850	0·51926
June............	29 11 23 A.M.	63·1	4·6538		
	2 57 P.M.	72·5	4·6523	0·30915	0·51971
July	29 11 28 A.M.	66·0	4·6572		
	2 56 P.M.	71·8	4·6540	0·30872	0·51965
August	28 11 20 A.M.	63·7	4·6541		
	2 48 P.M.	66·8	4·6563	0·30859	0·51955
September........	28 11 38 A.M.	55·0	4·6542		
	3 17 P.M.	61·7	4·6522	0·30857	0·51922

* A vibration is a movement of the magnet from a position of maximum displacement on one side of the meridian to a corresponding position on the other side.

† *m* = magnetic moment of vibrating magnet.

Observations of Deflection for Absolute Measure of Horizontal Force.

Table II.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature. Fahr.	Observed Deflection.	Log $\frac{m}{X}$. Mean.
1884.	d. h. m.	foot.			
October........	29 12 15 P.M.	1·0	51°·4	15° 21′ 56″	9·12360
		1·3	....	6 55 41	
	30 11 45 A.M.	1·0	50·7	15 21 28	
		1·3	....	6 55 48	9·12319
	3 0 P.M.	1·0	57·9	15 18 58	
		1·3	....	6 54 45	
November......	27 12 15 P.M.	1·0	47·7	15 19 54	
		1·3	....	6 55 6	9·12259
	2 15 „	1·0	49·4	15 19 53	
		1·3	....	6 54 56	
December......	29 12 36 P.M.	1·0	40·1	15 21 30	
		1·3	....	6 55 53	9·12264
	2 13 „	1·0	40·0	15 20 44	
		1·3	....	6 55 25	
1885.					
January........	29 2 47 P.M.	1·0	56·0	15 19 13	9·12283
		1·3	....	6 54 48	
	30 11 27 A.M.	1·0	49·6	15 20 48	9·12298
		1·3	....	6 55 13	
February	26 12 28 P.M.	1·0	53·0	15 19 22	
		1·3	....	6 54 44	9·12251
	2 23 „	1·0	54·2	15 18 34	
		1·3	....	6 54 34	
March	23 11 33 A.M.	1·0	45·0	15 21 3	
		1·3	....	6 55 9	9·12266
	2 49 P.M.	1·0	47·3	15 20 37	
		1·3	....	6 54 55	
	24 11 40 A.M.	1·0	45·7	15 20 58	
		1·3	....	6 55 15	9·12279
	2 44 P.M	1·0	52·5	15 20 2	
		1·3	....	6 54 50	
April..........	27 12 20 P.M.	1·0	65·6	15 18 34	
		1·3	....	6 54 14	9·12277
	2 12 „	1·0	69·8	15 16 45	
		1·3	....	6 53 25	
	28 2 58 „	1·0	72·2	15 17 13	9·12313
		1·3	....	6 54 5	
May	28 12 4 P.M.	1·0	71·2	15 19 14	
		1·3	....	6 54 26	9·12350
	2 12 „	1·0	74·0	15 17 16	
		1·3	....	6 54 10	
	29 3 22 „	1·0	70·5	15 16 10	9·12226
		1·3	....	6 53 7	
June	29 12 9 P.M.	1·0	64·8	15 17 33	
		1·3	....	6 53 48	9·12236
	2 17 „	1·0	72·5	15 15 21	
		1·3	....	6 53 4	

Table II—*continued.*

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature. Fahr.	Observed Deflection.	Log $\frac{m}{X}$. Mean.
1885.	d. h. m.	foot.	°	° ′ ″	
July	29 12 17 P.M.	1·0	67·6	15 17 42	
		1·3	....	6 54 7	9·12270
	2 14 „	1·0	71·4	15 16 6	
		1·3	....	6 53 25	
August	28 12 5 P.M.	1·0	64·4	15 17 4	
		1·3	....	6 54 19	9·12267
	2 9 „	1·0	65·9	15 17 12	
		1·3	....	6 54 7	
September......	28 12 37 P.M.	1·0	57·0	15 18 22	
		1·3	....	6 54 27	9·12213
	2 32 „	1·0	60·8	15 16 12	
		1·3	....	6 53 26	

Inclination Observations.—Table III.

Month.	G. M. T.	Needle.	Inclination.
			North.
1884.	d. h. m.	No.	° ′
Oct.	6 2 58 P.M.	1	67 39·31
	3 0 ,,	2	38·43
	23 2 33 ,,	1	38·59
	2 33 ,,	2	39·90
	24 2 30 ,,	1	39·25
	2 29 ,,	2	39·66
	Mean..	....	67 39·19
Nov.	25 2 46 P.M.	1	67 40·47
	2 47 ,,	2	40·31
	26 2 30 ,,	1	38·63
	2 29 ,,	2	38·90
	Mean..	....	67 39·58
Dec.	30 2 20 P.M.	1	67 39·03
	2 18 ,,	2	39·40
	31 2 33 ,,	1	38·25
	2 33 ,,	2	39·44
	Mean..	....	67 39·03
1885. Jan.	26 2 48 P.M.	1	67 38·46
	2 46 ,,	2	39·22
	28 2 37 ,,	1	39·50
	2 36 ,,	2	39·56
	Mean..	....	67 39·18
Feb.	23 2 45 P.M.	1	67 37·53
	2 43 ,,	2	38·87
	24 2 35 ,,	1	38·68
	2 35 ,,	2	37·68
	25 2 49 ,,	1	36·88
	2 49 ,,	2	37·40
	Mean..	...	67 37·84
Mar.	25 2 48 P.M.	1	67 36·96
	2 45 ,,	2	37·53
	26 2 46 ,,	1	38·06
	2 44 ,,	2	38·85
	Mean..	....	67 37·85

Month.	G. M. T.	Needle.	Inclination.
			North.
1885.	d. h. m.	No.	° ′
April	20 2 45 P.M.	1	67 38·44
	2 51 ,,	2	38·25
	21 2 47 ,,	1	37·06
	2 47 ,,	2	37·22
	24 3 2 ,,	1	36·41
	3 3 ,,	2	37·13
	Mean..	....	67 37·42
May	26 3 8 P.M.	1	67 40·69
	3 9 ,,	2	38·10
	27 2 34 ,,	1	40·25
	2 33 ,,	2	40·03
	30 2 43 ,,	1	39·00
	2 42 ,,	2	39·06
	Mean..	....	67 39·52
June	23 2 40 P.M.	1	67 38·18
	2 42 ,,	2	38·28
	25 2 52 ,,	1	37·26
	2 47 ,,	2	37·53
	Mean..	....	67 37·81
July	27 3 24 P.M.	1	67 37·97
	3 24 ,,	2	37·22
	28 3 4 ,,	1	37·03
	3 4 ,,	2	37·06
	Mean..	....	67 37·32
Aug.	25 2 48 P.M.	1	67 37·62
	2 45 ,,	2	36·59
	27 3 52 ,,	1	36·09
	3 52 ,,	2	37·62
	28 3 51 ,,	1	38·25
	3 52 ,,	2	37·34
	Mean..	....	67 37·25
Sept.	21 3 40 P.M.	1	67 36·09
	3 40 ,,	2	37·34
	22 2 44 ,,	1	38·18
	2 44 ,,	2	36·40
	25 3 18 ,,	1	39·31
	3 16 ,,	2	39·90
	Mean..	....	67 37·87

Table IV.

Month.	Declination.	Magnetic Intensity.								
	Mean of Observations.	English Units.			Metric Units.			C. G. S. Units.		
		X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1884.	° ′ ″									
October	18 29 25	3·9125	9·5177	10·2903	1·8040	4·3884	4·7447	0·1804	0·4388	0·4745
November	18 29 3	3·9195	9·5376	10·3103	1·8072	4·3976	4·7539	0·1807	0·4398	0·4754
December	18 27 46	3·9168	9·5267	10·3005	1·8060	4·3926	4·7494	0·1806	0·4393	0·4749
1885.										
January	18 25 37	3·9150	9·5238	10·2970	1·8052	4·3913	4·7478	0·1805	0·4391	0·4748
February......	18 27 25	3·9196	9·5238	10·2989	1·8072	4·3913	4·7486	0·1807	0·4391	0·4749
March	18 26 0	3·9175	9·5190	10·2934	1·8063	4·3890	4·7462	0·1806	0·4389	0·4746
April	18 26 48	3·9158	9·5117	10·2863	1·8056	4·3857	4·7429	0·1806	0·4386	0·4743
May..........	18 24 15	3·9151	9·5264	10·2994	1·8052	4·3925	4·7489	0·1805	0·4392	0·4749
June	18 25 38	3·9210	9·5275	10·3027	1·8079	4·3930	4·7504	0·1808	0·4393	0·4750
July..........	18 25 56	3·9175	9·5150	10·2899	1·8063	4·3872	4·7445	0·1806	0·4387	0·4745
August	18 28 10	3·9171	9·5133	10·2882	1·8061	4·3864	4·7437	0·1806	0·4386	0·4744
September	18 25 1	3·9194	9·5238	10·2989	1·8072	4·3913	4·7487	0·1807	0·4391	0·4749

Meteorological Observations.—Table I.
Mean Monthly results.

Months.	Thermometer.*								Barometer.†					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1884.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.…	48·9	55·6	41·4	48·5	62·4	16 2 P.M.	33·9	29 6 A.M.	30·086	30·677	5 {8 A.M. / 9 ,,}	29·367	{9 11 P.M. / 10 2 A.M.}	·277
Nov.…	42·6	47·2	37·2	42·2	59·2	2 3 ,,	25·6	25 4 ,,	30·172	30·559	10 10 ,,	29·725	28 3 P.M.	·227
Dec.…	41·6	45·6	37·2	41·4	55·0	3 Noon	26·9	31 5 ,,	29·885	30·345	31 Midt.	28·848	20 5 A.M.	·214
1885.														
Jan.…	37·1	40·4	33·5	37·0	52·4	29 10 A.M.	25·4	22 5 ,,	29·908	30·431	7 10 A.M.	28·909	11 6 ,,	·185
Feb.…	43·9	49·0	39·1	44·1	56·3	12 {1 P.M. / 2 ,,}	27·6	21 7 ,,	29·729	30·225	21 11 ,,	29·076	16 8 P.M.	·239
March..	40·3	47·4	33·6	40·5	59·0	20 {2 ,, / 3 ,,}	25·0	8 6 ,,	30·089	30·608	14 Noon	29·262	6 7 A.M.	·184
April…	47·2	55·4	39·5	47·5	70·2	20 4 ,,	30·3	5 {3 ,, / 4 ,,}	29·798	30·327	19 10 A.M.	29·216	6 5 ,,	·231
May…	49·2	57·2	42·3	49·8	70·3	28 2 ,,	33·4	8 5 ,,	29·813‡	30·240§	12 {2 ,, / 3 ,,}	29·156	22 10 ,,	·259
June…	58·8	67·1	49·9	58·5	79·5	4 4 ,,	41·7	11 5 ,,	30·040	30·405	11 7 ,,	29·525	20 7 ,,	·357
July…	63·0	73·0	53·7	63·4	85·4	26 4 ,,	47·6	2 5 ,,	30·178	30·433	22 {7 ,, / 9 ,,}	29·852	19 4 ,,	·382
Aug.…	57·9	66·6	50·5	58·6	76·4	17 {2 ,, / 3 ,,}	42·1	14 5 ,,	29·981	30·349	14 9 ,,	29·640	10 7 P.M.	·341
Sept.…	55·0	62·4	47·7	55·1	73·0	15 1 ,,	33·8	27 4 ,,	29·895	30·359	22 10 ,,	29·191	11 3 A.M.	·344
Means..	48·8	55·6	42·1	48·9	..	....	..	....	29·965	..	....	..	....	·270

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council.

* The thermometers are 10 feet above the ground.
‡ Mean for one day is approximate.
† Readings reduced to 32° F., and to sea-level.
§ Approximate reading.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *.			Weather. Number of days on which were registered							Wind †. Number of days on which it was									
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.	Calm.
1884.		in.	in.																		
October ..	6·4	1·115	0·590	9	9	..	1	..	7	13	..	4	1	2	1	1	7	7	4	4	5
November	7·0	1·770	0·705	5	11	2	..	..	5	12	1	6	1	3	1	3	3	3	5	5	6
December	7·8	2·135	0·365	5	18	1	..	..	1	20	2	2	4	3	1	1	8	8	1	3	2
1885.																					
January ..	7·9	1·535	0·390	31	13	1	..	1	3	21	2	2	4	6	6	4	2	4	2	1	6
February .	7·1	2·860	0·655	16	19	..	..	..	4	14	3	1	1	..	1	7	10	3	1	4	..
March ...	6·0	1·475	0·740	21	7	2	..	..	4	8	..	6	8	4	1	4	2	1	2	3	7
April	5·6	1·780	0·545	15	11	..	1	..	6	10	1	5	7	2	1	5	3	1	2	4	2
May	6·4	2·895	0·435	4	18	..	5	2	3	9	..	2	2	1	1	3	9	5	1	7	1
June	5·6	1·835	0·535	8	11	..	..	..	9	11	..	8	4	2	1	3	6	2	1	3	3
July	4·8	0·475	0·280	11	5	..	..	1	9	6	..	3	4	4	..	3	9	4	2	2	7
August ..	6·1	1·085	0·335	26	12	..	..	3	5	7	..	9	8	3	..	..	6	1	1	3	9
September	6·4	4·325	1·370	10	21	..	..	1	3	9	..	5	1	1	..	2	10	3	4	4	3
Totals..		23·285			155	6	7	8	59	140	9	53	45	31	14	36	75	42	26	43	51

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo.*)			Minimum temperature on the ground.			Horizontal movement of the Air.*			
	Total number of hours recorded.	Percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest	Date.	Mean.	Lowest.	Date.	Average hourly Velocity.	Greatest hourly Velocity.	Date.	Hour.
1884.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.		
October	86 30	26	8 42	13	94	110	3	36	23·9	29	8	32	28	11 A.M.
November	42 48	16	6 42	3	70	95	5	31	17·2	25	9	35	21	2 P.M.
December	23 36	10	3 42	19	60	81	5	34	20·4	31	15	37	7	11 P.M.
1885.														
January	15 36	6	4 12	7	55	75	27	29	17·3	22	13	38	31	11 A.M.
February	54 12	19	7 42	18, 24	79	98	24	34	17·8	21	13	38	3, 8	11 A.M., 7 P.M.
March	106 42	29	9 42	28	96	109	20	27	19·0	31	10	28	9, 27	Noon., Noon.
April	161 36	38	13 0	21	103	128	27	32	19·8	5	12	37	25	4 P.M.
May	240 0	41	12 0	24	115	132	31	36	25·2	12	10	28	29	3 P.M.
June	232 6	47	14 30	4	121	137	24	45	35·1	18	10	29	16	11 A.M.
July	243 42	49	14 12	6	126	139	11	47	38·0	9	9	25	19	2 P.M.
August	160 0	39	12 36	13	118	135	17	44	32·0	14	10	34	28	11 A.M.
September ..	125 18	33	9 24	15	113	126	3	42	26·8	27	9	28	11, 12	8 A.M., 9 P.M.

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days without spots.
1884.			
October	14	11	0
November	13	10	1
December	7	7	0
1885.			
January	6	6	0
February	16	17	0
March	20	7	1
April	18	8	0
May	17	14	0
June	13	8	0
July	20	10	0
August	15	8	0
September	11	6	0
Totals	170	112	2

APPENDIX III.

Results of Watch Trials. Performance of the Watches which stood highest in each class during the year.

Watch deposited by	Number of watch.	Balance spring, &c.	Mean daily rate.	Mean daily variation of rate.	Mean variation for 1° F.	Difference between pendant up and dial up.	Mean difference of rate: Between pendant up and pendant right.	Mean difference of rate: Between pendant up and pendant left.	Mean difference of rate: Between dial up and dial down.	Difference between extremes of daily rate.	Marks awarded for: Daily variation of rate.	Marks awarded for: Change of rate with positions.	Marks awarded for: Temperature compensation.	Total Marks. 0—100.	Character of test.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.					
E. F. Ashley, Clerkenwell ...	03267	Overcoil, fusee	−1·9	±0·4	0·04	−1·2	−0·1	−1·5	+2·0	4·75	31·8	37·0	17·3	86·1	Class A.
Kullberg, London	2901	Overcoil, fusee	+0·8	±0·5	0·04	−0·7	+0·2	+1·2	−0·9	7·5	30·2	36·9	17·3	84·4	,,
Baume & Co., London.........	2262	Overcoil, going barrel	+2·2	±0·4	0·07	−1·0	−1·5	−1·3	+2·1	8	31·4	36·5	15·3	83·2	,,
Baume & Co., London.........	2266	Overcoil, going barrel	−2·7	±0·6	0·03	−0·7	−1·8	−2·1	+1·5	5	28·0	36·2	18·0	82·2	,,
E. F. Ashley, Clerkenwell ...	03033	Overcoil, fusee	+0·4	±0·4	0·07	−0·8	−2·4	+0·1	−4·1	7	32·2	33·4	15·3	80·9	,,
Baume & Co., London.........	2259	Overcoil, going barrel	+0·5	±0·5	0·06	+1·1	+0·7	−1·8	+2·4	7·5	29·4	34·8	16·0	80·2	,,
D. Buckney, Clerkenwell ...	15017	Overcoil, going barrel	−0·2	±0·4	0·10	+2·0	+4·5	+1·0	−1·5	8	31·4	34·8	13·3	79·5	,,
G. Carley & Co., London......	46937	Cylindrical, fusee	+4·0	±0·4	0·12	+3·4	+2·0	+3·3	+1·2	7	32·0	35·4	12·0	79·4	,,
G. Carley & Co., London......	46991	Volute, going barrel...........	−5·7	±0·4	0·09	+2·5	...	...	...	6·5	...	...	...	...	Class B.
W. Holland, Rockferry	3552	Overcoil, going barrel	+3·0	±0·5	...	−1·0	...	...	...	2·5	...	...	...	...	Class C.
W. Holland, Rockferry	3340	Overcoil, fusee	−1·5	±0·7	...	−1·0	...	...	...	3	...	...	...	...	,,

APPENDIX IV.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Old Kew Thermometer Screen	1868
	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Dr. T. Thorpe, F.R.S.	Three Open Scale Standard Thermometers, Nos. 561, 562, and 563.	1879
	Tripod Stand	1883
Major Herschel, R.E., F.R.S.	Invariable Pendulums, Nos. 1821, 4, and 11, Shelton Clock, R.S. No. 34. Stands, and Accessories.	1881
Mr. R. W. Munro ..	Standard Straight-edge.....................	1881
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
Major-General Sir H. Lefroy, R.A., F.R.S.	Toronto Daily Registers for 1850-3	1885
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
Professor O. J. Lodge	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Mr. W. F. Harrison .	Condensing lens and copper lamp chimney ..	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885

Presents, November 19, 1885.

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"Contributions to the Chemistry of Chlorophyll." By EDWARD SCHUNCK, F.R.S. Received April 30, 1885. Read May 7, 1885.

Action of Acids on Chlorophyll.

Every one who has worked with chlorophyll must be familiar with the peculiar effect produced on the addition of acids to its solutions. If an alcoholic solution be taken, the colour of the solution changes when an acid is added from bright green to yellowish-green, and the spectrum at the same time undergoes alteration. After standing some time the solution gives a dark green deposit, which, after separation from the greenish-yellow liquid, shows when dissolved in boiling alcohol or ether, the spectrum of so-called "acid chlorophyll."

This change is attributed by some to a simple modification of the chlorophyll; others consider it due to the formation of products of decomposition. The latter view is, I have no doubt, the correct one.

In order to obtain the products due to the action of acids on chlorophyll, I find it best to use hydrochloric acid. Fresh green leaves of any kind are extracted, without undergoing any preliminary treatment, with boiling spirits of wine. The extract, which should be of an intense green, is poured off from the exhausted leaves, and allowed to stand for a day or two. During this time, a light green somewhat slimy deposit is formed, consisting chiefly of wax and fatty matters coloured by chlorophyll.

On examining this deposit closely, it will almost invariably be found to contain, interspersed in the mass, small, red, glistening crystals. These crystals consist of a body first observed by Hartsen,* and called by him, "*chrysophyll*," a name which it would be well to retain.† The deposit having been separated by filtration, a current of hydrochloric acid gas is passed into the dark green filtrate. This produces at once a dark green almost black voluminous precipitate,

* "Neue Untersuchungen über das Chlorophyll," "Chem. Centralblatt," 1875, S. 613.

† The erythrophyll of Bougarel and the crystallised xanthophyll of other chemists are doubtless identical with chrysophyll.

which increases in quantity on standing. After the precipitate has settled, the liquid appears greenish-yellow; it shows the spectrum of acid chlorophyll, due to the presence of colouring matters from the precipitate, these being not entirely insoluble in alcohol, but it also contains yellow colouring matters which darken the blue end of the spectrum, as well as other bodies extracted from the leaves which have no connexion with chlorophyll.

The dark green precipitate is now filtered off and washed with spirits until the filtrate appears nearly colourless. It contains besides impurities, which are chiefly of a fatty nature, two distinct colouring matters identical with the phyllocyanin and phylloxanthin of Fremy. These names I see no reason to change. They have been retained by Tschirch,* one of the latest writers on the subject. The method I adopt for separating the two substances is essentially the same as that of Fremy. I have tried other methods, but they have led to no result. The use of alkaline solvents is to be avoided, since both colouring matters are changed by the action of alkalis. The crude product of the action of the acid is first treated with ether, in which nearly the whole dissolves. The insoluble matter having been filtered off, the filtrate is mixed with about its own volume of fuming hydrochloric acid. The mixture, after being well shaken, is left to stand, when it separates into two layers, an upper yellowish-green one, containing phylloxanthin and a great part of the fatty matters, and a lower dark blue one, containing phyllocyanin. This is the experiment of Fremy, which is so often referred to in memoirs on chlorophyll, as having led him to the conclusion that chlorophyll was a compound or mixture of a blue and a yellow colouring matter. I propose first to give an account of phyllocyanin, the colouring matter of the blue layer.

The two liquids obtained in the manner just described are separated in the usual way, and the lower blue one is agitated with fresh ether, the process being repeated until the ether appears nearly colourless, and the phylloxanthin has been removed. After a short exposure, to allow the ether contained in it to evaporate, it is mixed with water, which produces a dark blue precipitate. This is filtered off, washed to remove hydrochloric acid, then dissolved in boiling glacial acetic acid. This solution on cooling gives a crystalline deposit of phyllocyanin, which is filtered off and recrystallised from acetic acid. If, on decomposing a little of it with boiling dilute nitric acid, nothing is left undissolved, the product may be considered pure, but if yellow oily drops appear on the surface of the boiling acid, the process of crystallisation from acetic acid must be repeated. The product is finally filtered off, washed with acetic acid, and dried.

By the process just described, I have obtained phyllocyanin from

* "Untersuchungen über das Chlorophyll," Berlin, 1884.

grass, from ivy leaves, from the leaves of the common thorn, and from the fronds of *Pteris aquilina*. No difference could be discerned between the different specimens obtained.

Properties of Phyllocyanin.

When dry, phyllocyanin has the appearance of a dark blue mass, which may easily be reduced to a fine powder. It resembles indigo, but when rubbed with a hard body it remains blue, and does not exhibit the coppery lustre which characterises indigo. Under a lens, small white glistening points are seen dotting the mass, produced by reflexion from crystalline surfaces. Examined under the microscope, it is found to consist almost entirely of elongated rhomboidal, or irregularly six-sided crystalline plates, which are generally opaque, but when very thin are translucent and then appear olive-coloured by transmitted light. Phyllocyanin is insoluble in water. It dissolves in boiling alcohol, but a great part of the substance dissolved separates out on the solution cooling, as a voluminous mass, consisting of microscopic crystals. It is more soluble in ether, glacial acetic acid, benzol, and carbon disulphide than in alcohol, but the best solvent is chloroform, which takes up large quantities of phyllocyanin even in the cold. A minute quantity of the substance imparts an intense colour to any of these solvents, especially chloroform. It is only on diluting largely that these solutions lose their opacity. They then appear of a dull green or olive colour, and show the well-known and often described spectrum of "acid chlorophyll," consisting of five bands of various intensity. The solutions fluoresce, but not so strongly as solutions of chlorophyll.

When the ethereal solution is mixed with concentrated hydrochloric acid, the whole of the phyllocyanin is taken up by the acid, yielding a dark greenish-blue solution, which sinks to the bottom. Should phylloxanthin be present, it will be found in the upper ethereal layer, which then shows a yellowish-green tint, and an absorption spectrum differing from that of phyllocyanin. The lower greenish-blue solution shows when sufficiently dilute five absorption bands, the spectrum, in fact, of the hydrochloric acid compound of phyllocyanin, which differs widely from that of phyllocyanin itself.

Phyllocyanin is almost insoluble in boiling petroleum ether, and nearly insoluble in ligroin. It dissolves easily in warm aniline.

Phyllocyanin contains nitrogen, but is free from sulphur.

Action of Heat.—When heated on platinum, phyllocyanin gives off an acid smell, then swells up considerably, evolving gas which burns with a smoky flame, and leaves a voluminous charcoal, which burns away slowly, a hardly visible trace of ash being left. When heated slowly between two watch-glasses, it swells up slightly, and

becomes charred. On the lower glass there is left a black mass which imparts no colour to boiling chloroform, and seems to be simply charcoal. On the upper glass, there is found a small quantity of brownish sublimate, which, under the microscope, is seen to consist partly of crystalline needles, partly of regular rhombic crystals which are honey-yellow by transmitted light.

When slowly heated in an air bath to 160°, phyllocyanin remains apparently unchanged. It is still completely soluble in chloroform, the solution showing the usual absorption bands. On heating, however, to 180°, complete decomposition takes place, but without any change of volume in the substance. The charred mass now imparts to boiling chloroform only a very faint green tinge, and when heated in a tube, gives off no empyreumatic products, only a slight odour of hydrocyanic acid.

Action of Oxidisers.—On adding a little nitric acid to a boiling saturated solution of phyllocyanin in glacial acetic acid, the solution immediately becomes yellow, but without evolution of nitrous fumes. It deposits nothing on cooling and standing. On the addition of water it gives a dirty green flocculent precipitate, the filtrate from which is still yellow, but shows no absorption bands. The precipitate dissolves easily in alcohol, giving a yellow solution, which shows a spectrum differing from that of phyllocyanin.

Phyllocyanin itself, treated with boiling dilute nitric acid is rapidly decomposed and dissolved with evolution of nitrous fumes. The solution evaporated in the water-bath leaves a residue which, treated with water, dissolves in part. The filtrate has a bitter taste, and leaves, on evaporation, a soft yellow residue, in which, on standing, some colourless crystalline needles are formed. The portion insoluble in water dissolves easily in alcohol, giving a yellow solution which, on evaporation, leaves a residue having a crystalline appearance, but not really crystalline when examined under the microscope.

A hot concentrated solution of phyllocyanin in glacial acetic acid becomes, on the addition of a little chromic acid, yellowish-green, but deposits nothing on cooling. The solution gives with water a dull green precipitate, the filtrate from which is yellow, and shows no absorption bands, while the precipitate itself, treated with ether, gives a solution which differs somewhat as regards colour and absorption bands from a solution of phyllocyanin.

Phyllocyanin treated with a watery solution of chromic acid, to which a little sulphuric acid has been added, is decomposed with much effervescence, giving a green solution, which, after evaporation, leaves a residue yielding to solvents only amorphous products.

Insolation.—I will introduce here what I have to say regarding the effect of insolation on phyllocyanin, because there can be no doubt

that the effects observed are due to oxidation. Anyone who has observed the ease and rapidity with which a solution of chlorophyll is bleached on exposure to light and air would be struck with the extraordinary permanence exhibited by phyllocyanin under the same circumstances. A moderately strong solution of phyllocyanin when exposed to sunlight retains its colour for a long time, the last trace disappearing only after many weeks' exposure.

In order to observe the changes which take place, it is best to take a chloroformic solution of phyllocyanin and expose it to sunlight in a loosely-stoppered bottle, the stopper being occasionally removed, and the contents shaken. The green colour of the solution gradually becomes fainter, the absorption bands of phyllocyanin remaining visible. After some time the colour changes to yellow, but the solution still shows a strong band in the red, corresponding to band I of phyllocyanin. At length this band also disappears, and there is now nothing to be seen but the total obscuration at the blue end of the spectrum, which most yellow solutions show. In one experiment the chloroformic solution was exposed to alternate bright sunlight and diffused daylight from the middle of April to the middle of June, when it was filtered from a flocculent yellow deposit which had formed. A further exposure to the middle of August was required to cause the entire disappearance of the band in the red.

Several products are formed during this process of insolation. The first consists of a flocculent yellow deposit, which gradually separates from the chloroformic solution. This deposit, after filtering off and washing with chloroform, is found to be easily soluble in alcohol and caustic alkali, but insoluble in boiling water, in which it simply melts; insoluble also in ether, ligroin, and carbon disulphide. The alcoholic solution is yellow, shows no absorption bands, and leaves, on spontaneous evaporation, a residue which has a somewhat crystalline appearance, but is found to be amorphous when examined under the microscope. The chloroformic filtrate leaves, on evaporation, a residue, which, on treatment with hot water, dissolves only in part. The part left undissolved by water resembles the product deposited from the chloroformic solution. It melts in boiling water; it is easily soluble in alcohol, the solution being yellow and leaving, on evaporation, a pale yellow brittle amorphous residue; it is insoluble in ether and carbon disulphide. The watery filtrate from this second product, after treatment with animal charcoal, which deprives it of most of its colour, leaves, on evaporation over sulphuric acid, a pale yellow transparent gum-like residue, in which nothing crystalline can be detected, and of which the following are the most characteristic reactions:—

Heated on platinum it is decomposed, giving off acid fumes with an odour like those from heated tartaric acid, leaving much charcoal;

its watery solution has a strong acid reaction and a sour and at the same time very bitter taste; the solution gives no coloration with ferric chloride; it evolves ammonia when boiled with caustic potash, and it reduces Fehling's solution on boiling.

It appears, therefore, that by insolation, phyllocyanin yields products which resemble, if they are not identical with, those due to the action of nitric and chromic acids. It is possible that some of these products may be contained in faded autumnal leaves after the chlorophyll has disappeared, but their indefinite character would render their identification very difficult. Green leaves, in becoming yellow, pass through a stage in which they yield when treated with alcohol an extract which, though quite yellow and non-fluorescent, shows a strong absorption band in the red. Of this fact I was forcibly reminded when watching the progressive changes of phyllocyanin during insolation.

Action of Chlorine.—On passing a current of chlorine gas through a chloroformic solution of phyllocyanin, the first effect observed is a change of colour in the solution from dull green to a bright grass-green, the latter colour closely resembling that of a solution of chlorophyll. The solution shows an absorption spectrum which coincides neither with that of chlorophyll nor with that of phyllocyanin.

On standing for a day or two the solution loses its bright green colour, and acquires a reddish hue, with a green tint at the edges where the thickness of the liquid is less. It now shows the same number of bands as a phyllocyanin solution, but the bands are all nearer the red end. On evaporation it leaves a greenish-brown amorphous residue. On passing more chlorine through the chloroformic solution of phyllocyanin the green colour seen at first disappears, the solution becomes yellow, at last pale yellow, all the bands characteristic of phyllocyanin at the same time disappearing. The liquid leaves, on evaporation, a yellow amorphous residue like resin. This, after heating in the water-bath to drive away all traces of hydrochloric acid, is found to contain chlorine. It does not dissolve in boiling water, but merely softens. It is easily soluble in alcohol, the solution being yellow and showing much obscuration in the blue of the spectrum, but no absorption bands even when very dilute. It dissolves only in part when treated with caustic potash lye.

Action of Bromine.—On the addition of a little bromine to a chloroformic solution of phyllocyanin, the solution acquires a bright grass-green colour, and now shows four absorption bands. On adding an excess of bromine to the solution, heating and evaporating, an olive-coloured amorphous product, containing bromine, is obtained, which dissolves easily in alcohol. The solution is brownish-red, and shows a spectrum similar to that of phyllocyanin, but having the bands all nearer the red end.

Action of Acids.—A mixture of 1 part of concentrated hydrochloric acid with 9 parts of absolute alcohol, dissolves phyllocyanin easily. In daylight the solution appears dark blue, green at the edges; in artificial light it appears purple. It shows a spectrum differing from that of phyllocyanin, and more nearly resembling that of chlorophyll, especially as regards the fourth and fifth bands, which are extremely faint, whereas with phyllocyanin they are very intense; the spectrum is, in fact, that of the hydrochloric acid compound of phyllocyanin. On adding water to the solution, unchanged phyllocyanin is precipitated, but if the solution be evaporated, it leaves a residue green by transmitted, blue by reflected light, which is no longer phyllocyanin, for it dissolves in alcohol with a brownish-green colour, and the solution shows a spectrum identical with that of the product formed by the action of alkalis on phyllocyanin, and characterised by two bands in the red, and two fine, but distinct bands in the green, which will be referred to presently.

Phyllocyanin is soluble in concentrated hydrobromic acid.

It dissolves in concentrated sulphuric acid, giving a grass-green solution, which shows a spectrum resembling that of the hydrochloric acid compound. On the addition of water, unchanged phyllocyanin is precipitated, but if the solution be left to stand for some time, the phyllocyanin contained in it is changed, and the precipitate with water now consists of several products, one of which shows the same spectrum as that formed by the action of hydrochloric acid.

Phyllocyanin is not in any way affected by treatment with a boiling watery solution of phosphoric acid. On the addition of phosphoric acid to a boiling alcoholic solution of phyllocyanin, the latter acquires a purplish tint, but the spectrum remains the same, and the solution on cooling deposits unchanged phyllocyanin.

Phyllocyanin shows no tendency to combine with oxalic, tartaric, or citric acid, but these acids do affect it to a certain degree, that is, they induce decomposition at temperatures at which the substance itself remains unchanged. Mixtures of finely powdered phyllocyanin with oxalic, tartaric, and citric acids remain unaltered when heated in the water-bath. After being heated in an air-bath to 130°, the oxalic acid mixture no longer contains phyllocyanin, the latter being completely charred and decomposed, whereas the tartaric and citric acid mixtures show only slight indications of change at that temperature. On being heated to 155°, and kept at that temperature for some time, the tartaric and citric acid mixtures are found to contain products which differ from phyllocyanin, without having properties such as compounds of the latter with acids might be expected to show. Under the conditions described, therefore, phyllocyanin shows no tendency to combine with weak acids.

Action of Alkalis.—When phyllocyanin is treated with very dilute

caustic potash or soda-lye, it dissolves entirely. The solution has the same colour as other phyllocyanin solutions, and shows a similar spectrum, but with the bands less distinctly marked. It gives precipitates of various shades of green, with earthy and metallic salts, such as the chlorides of barium and calcium, lead acetate, and copper acetate. These might, perhaps, be regarded as compounds of phyllocyanin, and be called phyllocyanates. Nevertheless it is easy to show that by mere solution in alkali, phyllocyanin undergoes a complete change. If the alkaline solution be mixed with an excess of acetic acid, and then shaken up with ether, the precipitate with acid dissolves in the ether, the solution having the colour of a phyllocyanin solution, and showing the characteristic absorption-bands; but if the solution be left to stand in contact with excess of acid for some time, its colour changes from olive to a brown or smoke colour, and it now shows quite a different and very elegant spectrum, characterised by two bands in the red, one of which is thin and nearer the red end than the first band of phyllocyanin, and two very fine but distinct bands in the green, the third and fourth bands of phyllocyanin having disappeared, while the fifth still remains. Since phyllocyanin, before solution in alkali, is not changed appreciably by acetic acid, even on boiling, it is evident that by the action of alkali it is in some way metamorphosed, so as to be liable to further change when acted on by the acid. In preparing this product of the successive action of alkali and acid, care must be taken to operate in the cold, for if hot alkaline lye be used, an entirely different product is formed. It is deposited from a boiling alcoholic solution in microscopic crystals, which are quite opaque, even in a strong light, and resemble crystallised phyllocyanin. In none of the various memoirs on chlorophyll that have come under my notice have I seen any spectrum figured or described at all resembling that of the solutions of this substance. The spectrum is distinctly seen, even in exceedingly dilute solutions.

The next product of the action of alkalis on phyllocyanin is formed when hot alkaline lye is employed. In order to obtain it, a solution of phyllocyanin in boiling alcohol is mixed with alcoholic potash or soda, and boiled. On standing, the solution yields a semi-crystalline deposit of a fine purple colour by reflected light, consisting of a potassium or sodium compound of the product formed. This is filtered off, washed with alcohol, and then dissolved in water. The solution gives with acetic acid a green flocculent precipitate, which is filtered off, washed, and dissolved in boiling glacial acetic acid. This solution on standing deposits small crystalline rosettes, which are green by transmitted light, and of a fine purple by reflected light. The solutions of this substance have a dull purple colour, and exhibit a distinct spectrum, characterised by a broad very dark band in the green.

I shall return to these products on a future occasion.

Phyllocyanin does not dissolve very readily in ammonia.

Action of Aniline.—When a solution of phyllocyanin in aniline is slowly evaporated, a residue is left consisting partly of dark granules of phyllocyanin, partly of pale purplish-brown crystalline needles, arranged in tufts and rosettes. The latter may be either a compound with aniline, or a product of the action of aniline on phyllocyanin at the ordinary temperature; it is formed in minute quantities only. A more energetic action takes place at higher temperatures. When aniline and phyllocyanin are heated together in a sealed tube to 130° and kept at that temperature for some time, a complete change takes place. The contents of the tube on being poured into alcohol dissolve in part only, a crystalline mass being left undissolved, the filtrate from which is greenish-brown, and shows the peculiar spectrum of the first product of the action of alkalis on phyllocyanin. The crystalline mass on the filter dissolves partly on treatment with boiling alcohol, and the filtered liquid deposits on cooling a quantity of white crystalline needles in star-shaped groups, which are soluble in ether and chloroform, but insoluble in dilute acids and alkaline lyes. The portion of the crystalline mass left undissolved by boiling alcohol dissolves in chloroform. The solution is red, and shows a very characteristic spectrum, consisting of three fine but very distinct bands in the red, of which the central one is the strongest, one very dark band covering the yellow and part of the green, which, when the solution is so far diluted as to show only two bands in the red, splits up into two nearly equal bands, and lastly, one dark band at the edge of the green and blue. The chloroformic solution leaves, on evaporation, a semi-crystalline residue, having a purplish hue.

It might be supposed that on treatment with ammonia, phyllocyanin would yield products similar to those formed by the action of aniline, but this is not the case. When phyllocyanin is heated with strong liquor ammoniæ in a sealed tube to 140°, bodies are formed which are similar to if not identical with those due to the action of fixed alkalis.

Compounds of Phyllocyanin.—From what has been stated above, it may be inferred that phyllocyanin plays the part of a weak base, that is, it combines with strong acids, the compounds however being unstable, and easily decomposed even by water. Like other weak bases, it may also act as an acid, though for reasons before mentioned, it may be doubted whether, in combining with bases, it does so without undergoing change. Notwithstanding its nearly neutral character, however, phyllocyanin is capable of yielding compounds of great comparative stability, into which metals and acids, especially organic acids, enter as constituents.

When phyllocyanin is dissolved in boiling glacial acetic acid, it

crystallises out unchanged on the solution cooling. The same happens when freshly precipitated cupric oxide or zinc oxide is added to a boiling alcoholic solution of phyllocyanin; the solution deposits phyllocyanin, and there are no indications of any combination taking place between the phyllocyanin and the metallic oxide.

A very different effect is observed when either of the two oxides is employed along with acetic acid. When cupric oxide is added to a solution of phyllocyanin in boiling acetic acid, the solution acquires at once a deep greenish-blue colour, and it no longer contains uncombined phyllocyanin, for its spectrum is different, and on standing it deposits lustrous crystals, which doubtless consist of a compound of which phyllocyanin, acetic acid, and copper are essential constituents. If zinc oxide be employed, a similar effect is observed; the liquid acquires an intense green colour, and now contains the corresponding acetate of phyllocyanin and zinc.

The same phenomenon is seen when ferrous oxide, manganese oxide, or silver oxide, or one of the corresponding acetates is taken, solutions of various shades of green being obtained, which contain phyllocyanin compounds, but no similar compounds are formed when potassium, sodium, barium, calcium, magnesium, or lead acetates are employed, for on adding the acetate of any of the last-named metals to an acetic acid solution of phyllocyanin, the colour of the latter remains unchanged, and phyllocyanin is deposited, just as if no metallic acetate were present. Acetic acid is, however, not the only acid which yields the reaction. If palmitic, stearic, oleic, tartaric, citric, malic, or phosphoric acid be employed, it takes place just as with acetic acid, but, in some cases, time is required for its completion. The process results, there can be little doubt, in the formation of compounds analogous to those with acetic acid. To obtain these compounds, it is only necessary to add a little of the freshly precipitated metallic oxide, and an excess of one of the acids named, to a solution of phyllocyanin in boiling alcohol, keeping the solution boiling for several hours, then filtering and adding water, in which the compounds are insoluble. The precipitated compounds, which are generally of a bright green, are filtered off and washed before treatment with reagents.

The following is an enumeration of the compounds obtained in the way just described:—

Phyllocyanin cupric acetate.
,, ,, palmitate.
,, ,, stearate.
,, ,, oleate.
,, ,, tartrate.
,, ,, citrate.
,, ,, phosphate.

Phyllocyanin argentic acetate.
,, zinc acetate.
,, ,, palmitate.
,, ,, stearate.
,, ,, oleate.
,, ,, citrate.
,, ferrous acetate.
,, ,, palmitate.
,, ,, oleate.
,, ,, citrate.
,, ,, malate.
,, ,, phosphate.
,, manganese acetate.

It would, of course, be easy to extend this list by taking a greater variety of acids and metallic oxides.

Nevertheless, strange to say, several compounds, the existence of which might have been anticipated, are not formed under the same conditions as those above enumerated. Phyllocyanin does not enter into combination when its alcoholic solution is boiled with cupric oxide and oxalic acid, zinc oxide and oxalic acid, zinc oxide and tartaric acid, ferrous oxide and tartaric acid. I could also see no indication of double compounds of phyllocyanin hydrochloride with the chlorides of platinum, mercury, or copper being formed, but on the other hand, a double sulphate of phyllocyanin and copper seems to exist. Attempts to form compounds by heating mixtures of phyllocyanin with glycerin and various fatty acids, at a temperature of 130°, led to negative results.

The various compounds above enumerated have a number of properties in common, though the several classes differ, *inter se*, in some important particulars.

They all dissolve more or less easily in alcohol, ether, chloroform, benzol and carbon disulphide, in fact in all the solvents which take up phyllocyanin and chlorophyll, but they are all insoluble in water, with the exception of the phyllocyanin manganese acetate, which dissolves readily therein. The solutions have a green colour, varying from grass-green, like that of chlorophyll solutions, to a fine bluish-green or blue, and they show peculiar spectra. The alcoholic solutions remain quite unchanged when sulphuretted hydrogen is passed through them, no precipitate is formed, and the solutions, on evaporation, leave the various compounds with their original properties unchanged. It is only on incineration that the presence of the metallic constituents is detected, the copper compounds leaving, after being burnt, cupric oxide; the zinc compounds, zinc oxide, the iron compounds, ferric oxide. Lastly, they are all soluble in dilute alkaline lyes, and are again precipitated unchanged on the addition of acetic

acid. These reactions make it somewhat doubtful whether these compounds are to be considered as double salts in the ordinary acceptation, and whether the metallic constituents may not rather be contained in them somewhat in the same way as the iron in hæmatin.

I have still a few remarks to make on the distinctive properties characterising the three principal groups of these compounds, the cupric, zinc, and ferrous groups.

Of these, the cupric compounds are the most beautiful, and, at the same time, the most stable. Their solutions show a brilliant colour, inclining more to blue than green, and spectra with four absorption bands, the position of which varies somewhat according to the acid employed. They are not decomposed by treatment with strong acids. If an alcoholic solution of any one of them be mixed with a large quantity of hydrochloric acid and boiled, the colour is not changed, and on adding water, and then shaking up with ether, the ethereal liquid which rises to the surface shows the same colour and the same spectrum as the original alcoholic solution. The phyllocyanin cupric acetate is the most beautiful of the series. It is best prepared by adding cupric acetate to a solution of phyllocyanin in boiling acetic acid. The crystalline mass which separates out on standing is filtered off, treated with dilute hydrochloric acid to remove any excess of cupric acetate that may be present, and then redissolved in glacial acetic acid boiling. The solution on cooling deposits the compound in crystalline scales, which are elongated, pointed at the ends, of a pale greenish-blue by transmitted light, and of a brilliant purple, with a semi-metallic lustre, by reflected light; it much resembles crystallised indigo-blue. The other cupric compounds yield only microscopic crystals.

The zincic group of compounds yield solutions of a brilliant green, showing spectra of five bands. These compounds are distinguished from the preceding by their instability in the presence of strong acids. If an alcoholic solution of any one of the zinc compounds be mixed with hydrochloric acid and boiled, its colour changes to blue. On now adding water, the blue colour disappears, and a precipitate is formed which on shaking up with ether dissolves. The ethereal solution has the colour and shows the absorption spectrum of phyllocyanin solutions. The zinc compounds are therefore decomposed by hydrochloric acid, yielding phyllocyanin again as one of the products of decomposition.

The behaviour of phyllocyanin towards zinc oxide in the presence of organic acids may serve to explain a peculiar phenomenon first observed by Church* and subsequently described by Tschirch. The former took chlorophyll that had become olive-brown on standing, and, acting on it with zinc powder in the water-oven, obtained a body

* "Chemical News," xxxviii, 168.

yielding bright green solutions, which he took to be regenerated chlorophyll. Tschirch acted on Hoppe-Seyler's chlorophyllan with zinc powder, and observed the same phenomenon, the conclusion at which he arrived being the same, viz., that chlorophyll is reproduced from chlorophyllan by the reducing action of zinc. I think, however, that what Tschirch obtained was, in reality, a zinc compound, and would have been formed just as well by using zinc oxide. Chlorophyllan is probably an impure substance, containing, it may be, some fatty acid, together with phyllocyanin, so that by the action of zinc oxide on the mixture a compound similar to those above described may be formed.

I have tried the experiment with the precipitate produced by treating a chlorophyll solution with hydrochloric acid, which probably differs very little from chlorophyllan, dissolving it in boiling alcohol, adding zinc oxide and boiling for some time, when a bright green liquid was obtained, which might have been taken for a solution of chlorophyll, but evidently contained a zinc compound of the same character as those formed directly from phyllocyanin by a similar process. The spectrum was identical with that of the zinc compounds from phyllocyanin.

The ferrous group of compounds yield solutions of a pure green, like that of chlorophyll. They are not, however, strikingly fluorescent, and when exposed to sunlight in open vessels they retain their colour unchanged for a long time. The group may be divided into two sub-groups: the first sub-group comprising compounds into which one of the fatty acids—acetic, palmitic, or oleic acid—enters as a constituent; the other sub-group, such as are formed by the action of citric, malic, or phosphoric acid. The members of the first sub-group show, in solution, the same spectrum, consisting of four ill-defined absorption bands, with much obscuration throughout. To the other group belongs a spectrum consisting also of four bands, which are, however, differently placed and more distinct than with the first sub-group. If a small quantity of hydrochloric acid be added to an alcoholic solution of any one of the iron compounds the solution acquires a blue tint, and the bands are now found to have shifted considerably towards the blue end. This effect may be due to the removal of a portion of the iron, and the formation of compounds with less iron. If a member of the second sub-group be taken, the phosphate or the malate for instance, then on the addition of hydrochloric acid a spectrum is obtained which closely resembles the ordinary chlorophyll spectrum as regards the number, position, and relative strength of the bands. The members of the first sub-group undergo a further change by treatment with an excess of hydrochloric acid and boiling, but the resulting product is not phyllocyanin.

All the members of the iron group of compounds undergo a peculiar change by the action of ether. When the alcoholic solution of any one of them is mixed with its own volume of ether, the green colour gradually fades and gives place to yellow. All the bands except that in the red disappear, while another fine band further in the red makes its appearance. At length the two bands in the red also vanish, and nothing is now seen but a considerable obscuration at the blue end, due to the presence of a yellow colouring matter in solution. The change resembles that which is seen on exposure of a chlorophyll solution to air and sunlight; it takes place, however, quite as readily in a closely stoppered bottle, kept in the dark, as in an open tube exposed to light. It is effected more rapidly by ordinary methylated ether than by pure ether. Benzol and acetone act in the same way as ether, but chloroform and carbon disulphide are without effect.

The phyllocyanin manganese acetate, which is obtained by adding manganese acetate to a solution of phyllocyanin in glacial acetic acid, differs from the compounds previously described by its solubility in water. It also dissolves easily in alcohol, but less readily in ether and benzol. Its solutions are green, like those of the iron compounds, and show a peculiar spectrum. It is not changed by treatment with boiling hydrochloric acid, and its alcoholic solution remains unaltered when mixed with ether or benzol.

December 10, 1885.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Dr. Archibald Geikie.
Sir Joseph Hooker.
Professor Huxley.
General Strachey.

Dr. John Anderson (elected 1879) was admitted into the Society.

Pursuant to notice, Professors Adolf Baeyer, Felix Klein, A. Kowalewski, and Sven Lovén were balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "Preliminary Results of a Comparison of certain simultaneous Fluctuations of the Declination at Kew and at Stonyhurst during the Years 1883 and 1884, as recorded by the Magnetographs at these Observatories." By the Rev. STEPHEN JOSEPH PERRY, F.R.S., Director of the Stonyhurst Observatory, and BALFOUR STEWART, LL.D., F.R.S., Professor of Physics at the Owens College, Manchester. Received October 31, 1885.

From a comparison made by Messrs. Sidgreaves and Stewart ("Proc. Roy. Soc.," October, 1868), between a few prominent simultaneous changes of declination at Kew and at Stonyhurst, it appeared that the ratio between the magnitudes of such changes was not constant, but depended to some extent upon the abruptness of the disturbance.

With the view of examining into this matter, we have made a somewhat more detailed comparison, selecting for this purpose some

of the best marked fluctuations during the years 1883 and 1884, both large and small, abrupt and non-abrupt.

There are two ways in which such a comparison may be made, the first of these being to measure the vertical difference in the declination curve between the two turning points of a fluctuation. This is the method which we have pursued in this investigation. It is, however, subject to the objection that the course of the curve between two such points is not precisely a straight line, and hence that this course embraces different values of abruptness.

On the whole, however, this method as we have used it appears to us to lead to definite, and we think not inaccurate, results. The other method would be to compare together the simultaneous rates of change of the declination at the two observatories, selecting for this purpose such portions of the records as present the appearance of constant slope, that is to say are straight lines.

This method we have not hitherto pursued, but it is possible that we may do so, and compare it with the other in a contemplated future research.

It is unnecessary to give a description of the magnetographs at the two observatories, suffice it to say that both declination magnets are as nearly as possible of the same size and weight, being about 5·5 inches long, 0·8 inch broad, and 0·1 inch thick.

The scale of the arrangement is, however, different at the two observatories in such a manner that at Kew 1 mm. of scale = 0·87′, while at Stonyhurst 1 mm. of scale = 1·13′. It would thus appear that equal vertical curve-differences at Kew and at Stonyhurst are to each other very nearly in the proportion of 1 to 1·3. This is the proportion which we shall use in the present paper.

For the Kew results, we are indebted to the kindness of the Kew Committee; of Mr. Whipple, Superintendent of the Kew Observatory; and of Mr. Baker, the magnetical assistant there.

In the following table (I) we have embodied the actual results of the various measurements:—

Table I.—Results of the various Experiments.

Running number.	Date.	Time of commencement.		Time of end.		Nature of change of westerly declination.	Amount of vertical change in curve-inches.	
		K.	S.	K.	S.		K.	S.
	1883.	h. m.	h. m.	h. m.	h. m.			
1	February 22	6 5 P.M.	6 5 P.M.	6 22 P.M.	6 21 P.M.	Decrease	1·50	1·60
2	,, ...	6 22 ,,	6 21 ,,	6 35 ,,	6 35 ,,	Increase	1·13	1·48
3	,, ...	6 35 ,,	6 35 ,,	6 44 ,,	6 42 ,,	Decrease	0·15	0·44
4	,, ...	6 44 ,,	6 42 ,,	6 58 ,,	6 56 ,,	Increase	0·30	0·38
5	,, 25.......	1 5 A.M.	1 5 A.M.	1 57 A.M.	1 55 A.M.	,,	1·34	1·33
6	,, ...	1 57 ,,	1 55 ,,	2 13 ,,	2 11 ,,	Decrease	0·38	0·45
7	,, ...	2 13 ,,	2 11 ,,	2 19 ,,	2 17 ,,	Increase	0·20	0·18
8	,, ...	2 19 ,,	2 17 ,,	2 40 ,,	2 39 ,,	Decrease	0·93	0·97
9	,, ...	2 40 ,,	2 39 ,,	2 52 ,,	2 49 ,,	Increase	1·06	1·34
10	,, ...	2 52 ,,	2 49 ,,	2 53 ,,	2 52 ,,	Decrease	0·04	0·06
11	,, ...	2 53 ,,	2 52 ,,	2 55 ,,	2 54 ,,	Increase	0·17	0·14
12	,, ...	2 55 ,,	2 54 ,,	2 56 ,,	2 56 ,,	Decrease	0·05	0·11
13	,, ...	2 56 ,,	2 56 ,,	3 1 ,,	2 59 ,,	Increase	0·42	0·31
14	,, ...	3 1 ,,	2 59 ,,	3 42 ,,	3 39 ,,	Decrease	1·67	1·56
15	July 14	2 10 P.M.	2 10 P.M.	2 15 P.M.	2 13 P.M.	Increase	0·08	0·13
16	,, ...	2 15 ,,	2 13 ,,	2 25 ,,	2 22 ,,	Decrease	0·04	0·17
17	,, ...	2 25 ,,	2 22 ,,	2 35 ,,	2 34 ,,	Increase	0·06	0·06
18	,, ...	2 35 ,,	2 34 ,,	2 45 ,,	2 42 ,,	Decrease	0·08	0·06
19	,, ...	2 45 ,,	2 42 ,,	2 50 ,,	2 46 ,,	Increase	0·08	0·09
20	,, ...	2 50 ,,	2 46 ,,	3 00 ,,	2 57 ,,	Decrease	0·27	0·25
21	,, ...	3 00 ,,	2 57 ,,	3 3 ,,	3 00 ,,	Increase	0·04	0·10
22	,, ...	3 3 ,,	3 00 ,,	3 21 ,,	3 19 ,,	Decrease	0·66	0·69
23	,, ...	3 21 ,,	3 19 ,,	3 34 ,,	3 31 ,,	Increase	0·33	0·41
24	August 18	10 5 ,,	10 5 ,,	10 33 ,,	10 34 ,,	,,	0·84	0·81
25	,, ...	10 33 ,,	10 34 ,,	11 0 ,,	11 0 ,,	Decrease	0·81	0·90

Running number.	Date.	Time of commencement. K.	Time of commencement. S.	Time of end. K.	Time of end. S.	Nature of change of westerly declination.	Amount of vertical change in curve-inches. K.	Amount of vertical change in curve-inches. S.
	1883.	h. m.	h. m.	h. m.	h. m.			
26	August 18	11 0 P.M.	11 0 P.M.	11 5 P.M.	11 4 P.M.	Decrease	0·07	0·07
27	,, ...	11 5 ,,	11 4 ,,	11 40 ,,	11 40 ,,	Increase	0·23	0·34
28	October 15	9 40 ,,	9 40 ,,	9 43 ,,	9 44 ,,	,,	0·07	0·07
29	,, ...	9 43 ,,	9 44 ,,	10 10 ,,	10 11 ,,	Decrease	0·49	0·51
30	,, ...	10 10 ,,	10 11 ,,	10 26 ,,	10 27 ,,	Increase	0·49	0·53
31	,, ...	10 26 ,,	10 27 ,,	10 49 ,,	10 49 ,,	Decrease	0·33	0·36
32	,, 17	3 3 A.M.	3 3 A.M.	3 5 A.M.	3 7 A.M.	Increase	0·02	0·02
33	,, ...	3 5 ,,	3 7 ,,	3 15 ,,	3 15 ,,	Decrease	0·10	0·13
34	,, ...	3 15 ,,	3 15 ,,	3 17 ,,	3 18 ,,	Increase	0·05	0·07
35	,, ...	3 17 ,,	3 18 ,,	3 30 ,,	3 30 ,,	Decrease	0·19	0·18
36	,, ...	3 30 ,,	3 30 ,,	3 40 ,,	3 40 ,,	Increase	0·29	0·28
37	,, ...	3 40 ,,	3 30 ,,	3 49 ,,	3 48 ,,	,,	0·13	0·09
38	,, ...	3 49 ,,	3 48 ,,	4 40 ,,	4 39 ,,	Decrease	0·42	0·69
39	,, ...	4 40 ,,	4 39 ,,	4 53 ,,	4 52 ,,	,,	0·43	0·44
40	,, ...	4 53 ,,	4 52 ,,	5 17 ,,	5 15 ,,	Increase	0·91	1·02
41	,, ...	5 17 ,,	5 15 ,,	5 45 ,,	5 44 ,,	,,	0·27	0·26
42	,, 19	7 35 P.M.	7 33 P.M.	7 39 P.M.	7 38 P.M.	,,	0·06	0·10
43	,, ...	7 39 ,,	7 38 ,,	7 55 ,,	7 55 ,,	Decrease	0·27	0·32
44	,, ...	7 55 ,,	7 55 ,,	8 20 ,,	8 19 ,,	Increase	0·39	0·47
45	,, ...	8 20 ,,	8 19 ,,	8 37 ,,	8 37 ,,	Decrease	0·16	0·23
46	,, ...	8 37 ,,	8 37 ,,	9 3 ,,	9 2 ,,	Increase	0·26	0·33
47	,, 20	6 13 ,,	6 14 ,,	6 25 ,,	6 27 ,,	Decrease	0·11	0·10
48	,, ...	6 25 ,,	6 27 ,,	6 34 ,,	6 35 ,,	Increase	0·13	0·10
49	,, ...	6 34 ,,	6 35 ,,	7 0 ,,	7 2 ,,	Decrease	0·54	0·46
50	,, ...	7 0 ,,	7 2 ,,	7 6 ,,	7 7 ,,	,,	0·17	0·19
51	,, ...	7 6 ,,	7 7 ,,	7 37 ,,	7 38 ,,	Increase	0·61	0·67
52	,, ...	7 37 ,,	7 38 ,,	7 50 ,,	7 50 ,,	Decrease	0·67	0·71
53	,, ...	7 50 ,,	7 50 ,,	8 10 ,,	8 12 ,,	Increase	0·54	0·59

Running number.	Date.	Time of commencement.		Time of end.		Nature of change of westerly declination.	Amount of vertical change in curve-inches.	
		K.	S.	K.	S.		K.	S.
	1883.	h. m.	h. m.	h. m.	h. m.			
54	October 20	8 10 P.M.	8 12 P.M.	8 14 P.M.	8 15 P.M.	Decrease	0·05	0·07
55	"	8 14 "	8 15 "	8 20 "	8 20 "	Increase	0·03	0·03
56	"	8 20 "	8 20 "	8 29 "	8 31 "	Decrease	0·40	0·41
57	November 2	4 50 "	4 49 "	5 3 "	5 3 "	Increase	0·37	0·43
58	"	5 3 "	5 3 "	5 18 "	5 18 "	Decrease	0·60	0·63
59	"	5 18 "	5 18 "	5 35 "	5 36 "	Increase	0·27	0·31
60	"	5 35 "	5 36 "	5 40 "	5 42 "	Decrease	0·03	0·04
61	"	5 40 "	5 42 "	5 55 "	5 56 "	Increase	0·15	0·14
62	" 22	1 13 "	1 13 "	1 20 "	1 20 "	Decrease	0·11	0·17
63	"	1 20 "	1 20 "	1 40 "	1 39 "	Increase	0·12	0·19
64	"	1 40 "	1 39 "	1 49 "	1 50 "	Decrease	0·17	0·22
65	"	1 49 "	1 50 "	1 58 "	1 56 "	Increase	0·57	0·46
66	"	1 58 "	1 56 "	2 9 "	2 9 "	Decrease	0·67	0·55
67	"	2 9 "	2 9 "	2 18 "	2 19 "	Increase	0·30	0·35
68	"	2 18 "	2 19 "	2 22 "	2 24 "	Decrease	0·15	0·23
69	"	2 22 "	2 24 "	2 25 "	2 27 "	Increase	0·03	0·07
70	"	2 25 "	2 27 "	2 32 "	2 35 "	Decrease	0·09	0·10
71	"	2 32 "	2 35 "	2 42 "	2 42 "	Increase	0·18	0·22
72	"	2 42 "	2 42 "	2 50 "	2 49 "	Decrease	0·14	0·15
73	"	5 8 "	5 7 "	5 22 "	5 22 "	"	0·20	0·25
74	"	5 22 "	5 22 "	6 9 "	6 8 "	"	0·56	0·53
75	"	6 9 "	6 8 "	6 35 "	6 32 "	Increase	0·57	0·60
76	"	6 35 "	6 32 "	6 55 "	6 55 "	Decrease	0·09	0·17
77	"	6 55 "	6 55 "	7 13 "	7 13 "	Increase	0·32	0·51
78	"	7 13 "	7 13 "	7 20 "	7 19 "	Decrease	0·16	0·34
79	"	9 18 "	9 18 "	9 35 "	9 34 "	Increase	0·24	0·27
80	"	9 35 "	9 34 "	9 57 "	9 55 "	"	0·18	0·22
81	"	11 0 "	10 58 "	11 20 "	11 18 "	Decrease	0·44	0·45

Running number.	Date.	Time of commencement. K.	Time of commencement. S.	Time of end. K.	Time of end. S.	Nature of change of westerly declination.	Amount of vertical change in curve-inches. K.	Amount of vertical change in curve-inches. S.
	1883.	h. m.	h. m.	h. m.	h. m.			
82	November 23	0 7 A.M.	0 6 A.M.	0 20 A.M.	0 19 A.M.	Decrease	0·52	0·60
83	„	0 20 „	0 19 „	0 30 „	0 28 „	Increase	0·44	0·61
84	„	0 30 „	0 28 „	0 43 „	0 43 „	Decrease	0·31	0·42
85	„	0 43 „	0 43 „	0 49 „	0 49 „	Increase	0·14	0·25
86	„	0 49 „	0 49 „	0 54 „	0 53 „	Decrease	0·09	0·17
87	„	0 54 „	0 53 „	1 2 „	1 0 „	Increase	0·15	0·21
88	„	1 2 „	1 0 „	1 13 „	1 15 „	Decrease	0·28	0·34
89	„	1 13 „	1 15 „	1 28 „	1 28 „	Increase	0·41	0·43
90	„	4 13 „	4 12 „	4 35 „	4 34 „	Decrease	0·33	0·29
91	„	4 35 „	4 34 „	5 42 „	5 41 „	Increase	0·78	0·75
92	„	11 3 P.M.	11 4 P.M.	11 30 P.M.	11 30 P.M.	Decrease	0·51	0·51
93	December 17	8 15 „	8 16 „	8 56 „	8 57 „	„	0·97	0·95
94	„	8 56 „	8 57 „	9 2 „	9 3 „	Increase	0·08	0·08
95	„	9 2 „	9 3 „	9 10 „	9 13 „	Decrease	0·11	0·13
96	„	9 10 „	9 13 „	9 35 „	9 35 „	Increase	0·53	0·57
97	„ 18	6 50 „	6 48 „	7 15 „	7 13 „	Decrease	0·25	0·27
98	„	7 15 „	7 13 „	7 34 „	7 32 „	Increase	0·31	0·38
99	„ 25	4 10 „	4 10 „	4 15 „	4 15 „	„	0·01	0·02
100	„	4 15 „	4 15 „	4 20 „	4 20 „	Decrease	0·02	0·04
101	„	4 20 „	4 20 „	4 48 „	4 48 „	„	0·51	0·48
102	„	4 48 „	4 48 „	5 0 „	5 0 „	Increase	0·23	0·24
103	„	5 0 „	5 0 „	5 30 „	5 30 „	„	0·12	0·13
	1884.							
104	February 29	9 32 „	9 33 „	10 8 „	10 7 „	Decrease	0·97	0·95
105	July 3	9 20 „	9 18 „	9 31 „	9 30 „	„	1·11	1·18
106	„	9 31 „	9 30 „	9 40 „	9 39 „	Increase	0·70	0·99
107	„	9 40 „	9 39 „	9 44 „	9 44 „	Decrease	0·09	0·42
108	„	9 44 „	9 44 „	9 57 „	9 56 „	Increase	1·06	1·33

Running number.	Date.	Time of commencement.		Time of end.		Nature of change of westerly declination.	Amount of vertical change in curve-inches.	
		K.	S.	K.	S.		K.	S.
	1884.	h. m.	h. m.	h. m.	h. m.			
109	July 3	9 57 P.M.	9 56 P.M.	10 7 P.M.	10 5 P.M.	Decrease	1·03	1·37
110	„	10 7 „	10 5 „	10 15 „	10 13 „	Increase	0·49	0·86
111	„	10 15 „	10 13 „	10 21 „	10 20 „	Decrease	0·18	0·48
112	„	10 21 „	10 20 „	10 31 „	10 29 „	Increase	0·57	0·73
113	„	10 31 „	10 29 „	10 41 „	10 40 „	Decrease	0·48	0·61
114	„	0 38 A.M.	0 38 A.M.	1 15 A.M.	1 15 A.M.	Increase	1·32	1·30
115	September 17–18 ..	11 48 P.M.	11 46 P.M.	0 13 „	0 10 „	Decrease	0·78	0·98
116	October 3	2 50 A.M.	2 49 A.M.	4 3 „	4 2 „	Increase	0·86	0·85
117	November 2	7 1 P.M.	7 4 P.M.	7 13 P.M.	7 15 P.M.	Decrease	0·96	1·07
118	„	7 13 „	7 15 „	7 19 „	7 21 „	Increase	0·80	0·99
119	„ 3	1 39 A.M.	1 40 A.M.	2 13 A.M.	2 14 A.M.	„	1·51	1·43
120	December 14	8 32 P.M.	8 34 P.M.	9 36 P.M.	9 37 P.M.	Decrease	0·99	0·94
121	„	9 36 „	9 37 „	10 20 „	10 20 „	Increase	0·79	0·77
122	„ 22	10 13 „	10 13 „	10 37 „	10 39 „	„	0·94	1·02
123	„	10 37 „	10 39 „	11 10 „	11 11 „	Decrease	1·06	1·14

We do not know of a single instance in which the fluctuation is not in the same direction at both observatories.

We have given the G.M.T. of the commencement and end of each fluctuation at each observatory. Practically speaking, the times at both places are so nearly simultaneous that we do not feel justified in asserting that they are not quite so. Occasionally, however, there are indications that certain short period fluctuations are not precisely of the same duration at both places. In what follows we have rejected such cases; also we have adopted the durations as recorded at the Kew Observatory, rejecting however all cases when these are less than five minutes, inasmuch as an accurate measure of duration is essential to our method.

Let us now, simply as a conjecture which may be of service in indicating the best method of treating the observations of Table I, suppose that in these disturbances two causes are in operation, and that the result is due partly to true magnetic changes, and in part to secondary currents caused by these changes.

Let K denote the whole observed value of the disturbance at Kew, and of this let k denote the portion due to strictly magnetic change, also let $\alpha k\phi(t)$ be the portion of the whole disturbance caused by secondary action, α being a constant which may conceivably be either positive or negative, and t denoting the duration. Hence $K = k(1+\alpha\phi(t))$. In like manner let S denote the whole Stonyhurst change.

We are, perhaps, justified in putting $S = k(\beta + \gamma\phi(t))$, β and γ being constants.

Hence we shall have $\frac{S}{K}=\frac{\beta+\gamma\phi(t)}{1+\alpha\phi(t)}$, that is to say, $\frac{S}{K}$ will be a function of the duration.

It thus appears that the value of $\frac{S}{K}$ will, according to this or indeed according to any probable hypothesis of this nature, be independent of the values of S and K, and be a simple function of the duration.

These reasons have induced us to construct the following table (II), in which the ratio $\frac{S}{K}$ is ascertained for disturbances of varying durations.

Table II.—Value of $\frac{S}{K}$ for Disturbances of different Duration.

Duration in minutes	5. K.	5. S.	6. K.	6. S.	7. K.	7. S.	8. K.	8. S.	9. K.	9. S.	10. K.	10. S.	11. K.	11. S.	12. K.	12. S.	13. K.	13. S.	14. K.	14. S.
	42	31	20	18	11	17	14	15	—		4	17	111	118	11	10	113	148	30	38
	8	9	17	19	9	10	15	21	13	9	27	25	—		23	24	33	41	20	25
	7	7	3	3	16	34	49	86	13	10	29	28	—		96	107	19	18	—	
	3	4	14	25	—		—		30	35	44	61	—		—		43	44	—	
	9	17	8	8	—		—		70	99	103	137	—		—		67	71	—	
	1	2	18	48	—		—		—		57	73	—		—		37	43	—	
	2	4	80	99	—		—		—		48	61	—		—		52	60	—	
	—		—		—		—		—		—		—		—		106	133	—	
Sum..............	72	74	160	220	36	61	78	122	126	153	312	402	111	118	130	141	470	558	50	63
Reduced ratio	1·79								1·60						1·52					

Duration in minutes	15. K.	15. S.	16. K.	16. S.	17. K.	17. S.	18. K.	18. S.	19. K.	19. S.	20. K.	20. S.	21. K.	21. S.	22. K.	22. S.	23. K.	23. S.	24. K.	24. S.
	60	63	38	45	150	160	66	69	31	38	54	59	93	97	18	22	33	36	91	102
	15	14	49	53	16	23	32	51	—		12	19	—		33	29	26	33	94	102
	41	43	27	32	27	31	—		—		44	45	—		—		—		—	
	—		—		24	27	—		—		—		—		—		—		—	
Sum..............	116	120	114	130	217	241	98	120	31	38	110	123	93	97	51	51	59	69	185	204
Reduced ratio......	1·43						1·52						1·41							

Duration in minutes..	25.		26.		27.		28.		29.		30.		31.		32.		33.		34.	
	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.
	39	47	54	46	81	90	84	81	—		12	13	61	67	—		106	114	151	143
	25	27	57	60	49	51	27	26	—		—		—		—		—		—	
	78	98	—		51	51	51	48	—		—		—		—		—		—	
Sum...............	142	172	111	106	181	192	162	155	—		12	13	61	67	—		106	114	151	143
Reduced ratio	1·36										1·33									

Duration in minutes..	35.		36.		37.		38.		39.		40.		41.		42.		43.		44.		45.		46.		47.	
	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.
	23	34	97	95	132	130	—		—		—		167	156	—		—		79	77	—		—		56	53
	—		—		—		—		—		—		97	95	—		—		—		—		—		—	
Sum	23	34	97	95	132	130	—		—		—		264	251	—		—		79	77	—		—		56	53
Reduced ratio	1·34												1·24													

Duration in minutes..	48.		49.		50.		51.		52.		53.		54.		55.		56.		57.		58.		59.		60.	
	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.
Sum	—		—		—		62	69	134	133	—		—		—		—		—		—		—		—	
Reduced ratio..							1·34																			

Duration in minutes..	61.		62.		63.		64.		65.		66.		67.		68.		69.		70.		71.		72.		73.	
	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.	K.	S.
Sum	—		—		—		99	94	—		—		78	75	—		—		—		—		—		86	85
Reduced ratio							1·25																			

Table II will explain itself. In it we have embodied the various individual observations of Table I, with the following exceptions :—

On account of apparently unequal duration.		On account of the duration being under five minutes.
No. 3	....	No. 10
„ 9	.. ..	„ 11
„ 15	....	„ 12
„ 17	....	„ 21
„ 18	....	„ 28
„ 33	....	„ 32
„ 56	....	„ 34
„ 64	....	„ 42
„ 65	....	„ 54
„ 66	....	„ 68
„ 71	....	„ 69
„ 76	....	„ 107
„ 84		
„ 88		
„ 95		
„ 96		

From Table II we may deduce the following conclusions :—

(1.) *In the very great majority of cases the angular value of the declination disturbance is greater for Stonyhurst than for Kew.*

(2.) *The ratio* $\frac{S}{K}$ *is certainly greater for disturbances of short than for those of long duration. Our observations are not, however, sufficiently extensive to enable us to represent this ratio graphically as a function of the duration.*

(3.) *As far as we can tell from a limited number of observations the value of the above ratio does not depend on the magnitude of the disturbance.*

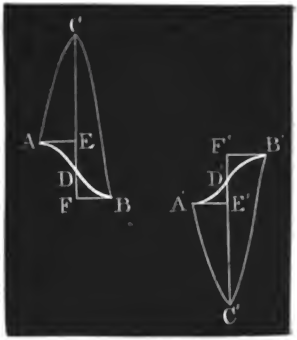
We trust to make on a future occasion a more complete comparison between the simultaneous magnetic fluctuations as derived from the curves of the two observatories.

NOTE.—It might be desirable to add a few words in fuller explanation of the method adopted.

This method is founded on the implied belief that disturbances are indications of the way in which the magnetic earth rights itself with regard to the forces acting upon it. Our experience is that such disturbances never occur singly, but very frequently as couplets or sets of couplets. There is no such thing as a magnetic tableland separated from another by a single slope. We have rather a rise and then a fall, or it may be a fall and then a rise, and in the end the state of things, after the disturbance has run its course, is not greatly different from that before it began. This duality, as well as the results of this paper, would lead us to imagine

that secondary currents must have an influence, perhaps a powerful one, in causing disturbances.

In order to fix the mind, let us here imagine that this secondary current influence (exhibited probably in the shape of an earth current) is opposed in direction to the true magnetic change. We should, therefore, expect something of the following nature.



ED or E′D′ = magnetic change, first movement.
DF or D′F′ = magnetic change, second movement.
DC or D′C′ = $\frac{\alpha k}{t}$.

In the first of these diagrams AB denotes a true magnetic descending change, while ACB is the observed disturbance couplet. In the second A′B′ denotes a true magnetic ascending change, while A′C′B′ is the observed disturbance couplet.

In our various measurements, therefore, it is assumed that we pass from a point of no disturbance, A or A′, to another, C or C′, in which there is a magnetic change and a superposed secondary current change, or from a point in which these two forces act to a final point, B or B′, of no disturbance. Now the *maximum* earth current force will depend upon the *maximum* rate of magnetic change. This maximum rate we cannot tell, but we may imagine it to be proportional to the mean rate of magnetic change, being possibly represented in an approximate manner by the expression—

Max. current force = a constant × $\frac{\text{magnetic change}}{\text{duration}}$. In other words, our general functions of the text would be replaced by the expressions (taking both branches of the curve)—

$$K = k\left(1 \mp \frac{\alpha}{t}\right)$$

$$S = k\left(\beta \mp \frac{\gamma}{t}\right)$$

It would appear from this as well as from the diagrams that the first turn of a couplet should be less than the second.

The results in our paper cannot, therefore, be regarded as a final analysis, but merely as being of sufficient interest to demand a fuller inquiry.—November 4th, 1885.

II. "On the Magnetisation of Steel, Cast Iron, and Soft Iron" (being the investigation for which the Watt Prize of 1884 was awarded by the Senate of the University of Glasgow). By JOHN W. GEMMELL. Communicated by Sir WILLIAM THOMSON, Kt., LL.D., F.R.S. Received October 31, 1885.

The experiments, of which the following is a description, with their results, were performed in the Physical Laboratory of the University of Glasgow, and had for their object the finding of the difference between specimens of iron and of steel with respect to the intensities of their total and residual magnetisation due to different degrees of magnetising force.

The specimens consisted of (1) wires of "soft Scotch" iron, of "common wire," of "charcoal iron," and of "soft steel;" and (2) bars of cast iron and of malleable iron. The wires were 31 cm. long and 0·5 cm. in diameter, and weighed respectively 39·85, 39·9, 41·5, and 38·5 grams. The bars, two of which were of cast iron procured from different foundries, were 15·25 cm. long, and of section 1 cm. square. The cast iron bars weighed each 114 grams, and the malleable iron bar 125 grams.

The arrangement of the apparatus employed in the investigation is shown in the accompanying diagram (fig. 1). The magnetising coil,

FIG. 1.

represented at C, was 40 cm. in length, and consisted of three layers of 600 turns each of silk-covered copper wire, wound on a brass tube of the same internal diameter as the wires. It was placed on a convenient stand with its axis horizontal and at right angles to the magnetic meridian. For the experiments on the bars the coil was 21 cm. in length, and consisted of five layers of 155 turns each of insulated

copper wire wound on a copper case of square section just fitting the bars.

The magnetising current passing through the coil was obtained from a battery of Thomson's tray Daniells, by means of which any desired current above 0·25 of an ampère could be employed. To obtain currents ranging from 0 to the minimum (0·25) to be got by the battery, a resistance-box, capable of inserting any resistance up to 10,000 ohms, was placed in the circuit with a single cell.

The strength of the current was measured by one of Sir William Thomson's graded galvanometers, represented at G, a full description of which will be found in Mr. Andrew Gray's "Absolute Measurements in Electricity and Magnetism."

A reflecting magnetometer, M, of the well-known form devised by Mr. J. T. Bottomley, was used to measure the intensity of magnetisation. In the experiments on the wires it was placed due magnetic east of the coil, at a distance of 1 metre from the middle point of its axis, and in such a position that if the axis were produced it would pass through the centre of the mirror. In the experiments on the bars its position was due magnetic north of the coil.

A framework, holding a lamp L, and having a scale S of half-millimetre divisions attached to it, was placed in front of the magnetometer at such a distance that the scale was exactly 1 metre from the magnetometer needle. The light from the lamp, passing through a tube T in front, in which a fine wire is vertically fixed, is reflected from the mirror to the scale, and the deflection read by the image of the fine wire in the middle of the spot of light.

The results in these experiments were got by beginning with a feeble magnetising current, which was increased step by step until the maximum obtainable was reached. It was then gradually diminished to zero, when the direction of the current was reversed, and the same process of increasing and diminishing repeated.

The magnetometer readings taken while the current is flowing represent the effect upon the magnetometer needle of the joint electromagnetic action of the current passing through the coil, and the magnetisation it produces in the wire or bar. Hence the effect due to the magnetisation of the wire or bar alone is obtained by subtracting from the total effect the magnetic effect of the coil. This last is proportional to the current flowing, and was found by experiment to be for the "wire" coil 0·0385, and for the "bar" coil 0·0326 of a division of the magnetometer scale per division of the galvanometer scale.

The results of the investigation are shown in the accompanying curves. The abscissæ are divisions of the galvanometer scale, and are therefore proportional to the magnetising forces. The ordinates are divisions of the magnetometer scale, and are therefore proportional to the magnetisation produced. The curves marked A represent the

total, and those marked B the residual magnetisation. Beginning at zero, we pass to the right, gradually increasing the magnetising force until we reach our limit. Then returning, we pass through zero, and with the opposite magnetising force proceed to the left limit, from which we again return to zero.

The first experiments were made upon the wires, the results of which are given in Curves I, II, III, and IV. These wires were afterwards annealed and retested, with the results shown in Curves V to VIII inclusive. The remaining Curves IX, X, and XI, contain the results obtained from the bars.

The results given by the curves may be reduced to absolute measure by means of the figures on each, which were obtained in the following manner:—

Let—

H = the horizontal component of the earth's magnetic force.
M = the magnetic moment of the wire or bar.
m = the magnetic moment of the magnetometer needle.
F = the strength of a pole of the wire or bar.
f = the strength of a pole of the needle.
r = the distance of the centre of the wire or bar from that of the needle.
a = half the distance between the poles of the wire or bar.
b = half the distance between the poles of the magnetometer needle.
θ = the angle of deflection of the magnetometer needle.

The position of the wire, with regard to the magnetometer in the experiments on the wires, is shown in fig. 2.

Fig. 2.

The pole N of the wire attracts the pole s of the needle with a force $\dfrac{Ff}{(r-a)^2}$, and the pole S repels the pole s with a force $\dfrac{Ff}{(r+a)^2}$, b being so small in comparison with r that the poles of the

needle may be regarded as at its centre c. Hence the total attractive force exerted on the pole s is—

$$\mathrm{F}f\left\{\frac{1}{(r-a)^2}-\frac{1}{(r+a)^2}\right\}, \text{ or } \mathrm{F}f\frac{4ar}{(r^2-a^2)^2}, \text{ that is } f\frac{2\mathrm{M}r}{(r^2-a^2)^2}.$$

We find similarly that the pole N exerts an equal repulsive force on the pole n. The needle is therefore acted on by a "couple"—

$$2bf\frac{2\mathrm{M}r}{(r^2-a^2)^2}\cos\theta, \text{ that is } \frac{2\mathrm{M}mr}{(r^2-a^2)^2}\cos\theta.$$

To balance this we have another "couple," $f\mathrm{H}\,.\,2b\sin\theta$, that is, $m\mathrm{H}\sin\theta$. Hence, equating these two couples, we get—

$$\mathrm{M}=\frac{(r^2-a^2)^2}{2r}\mathrm{H}\tan\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

The value of H at this point was found to be 0·16, by comparison with a particular spot in the laboratory, for which, by the method fully described in Mr. Thomas Gray's paper on "The Experimental Determination of Magnetic Moments in Absolute Measure" ("Philosophical Magazine," November, 1878), the value of H had already been very accurately determined. Half the distance between the poles of the wire, represented by a, may be taken as half its length, that is, 15·5 cm. The angle through which the magnetometer needle is deflected is measured on the half-millimetre scale; and for small angles $\tan\theta = \frac{1}{2}\tan 2\theta$; so $\tan\theta$ is got by dividing the scale reading by $2r$ in half-millimetres, that is by 4000. By substituting these values in equation (1), we find that the magnetic moments are obtained by multiplying the readings of the magnetometer scale, which in the curves are represented by the ordinates, by the factor 19·110125. The magnetic moments *per gram* are therefore got by multiplying the ordinates in—

Curves	I and	V	by	0·4795.
,,	II ,,	VI	,,	0·479.
,,	III ,,	VII	,,	0·4605.
,,	IV ,,	VIII	,,	0·4964.

The position of the bar with regard to the magnetometer in the experiments on the bars, is shown on fig. 3.

The pole N of the bar attracts the pole s of the needle with a force $\frac{\mathrm{F}f}{r^2+a^2}$, and the pole S repels the pole s with an equal force. The

resultant of these two forces is a force R, parallel to the bar, and of value $2Ff\dfrac{a}{(r^2+a^2)^{\frac{3}{2}}}$,* that is $f\dfrac{M}{(r^2+a^2)^{\frac{3}{2}}}$.

FIG. 3.

A force, equal and parallel to this, but in the opposite direction, may be similarly shown to act on the pole n of the needle. Thus the needle is acted on by a "couple," the value of which is—

$$2bf\frac{M}{(r^2+a^2)^{\frac{3}{2}}}\cos\theta, \text{ that is } \frac{Mm}{(r^2+a^2)^{\frac{3}{2}}}\cos\theta.$$

To balance this "couple" we have, as before, another "couple," $mH\sin\theta$. Hence, equating these, we get—

$$M=(r^2+a^2)^{\frac{3}{2}}H\tan\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Now, in these experiments, r is 100 cm., a 7·6 cm., and H was found to be 0·155. Substituting these values in equation (2), we

* $R^2=2\left(\dfrac{Ff}{r^2+a^2}\right)^2(1-\cos SsN)$; but $\cos SsN=\cos^2\tfrac{1}{2}SsN-\sin^2\tfrac{1}{2}SsN=\dfrac{r^2-a^2}{r^2+a^2}$; hence $R=2\dfrac{Ff}{r^2+a^2}\cdot\dfrac{a}{\sqrt{r^2+a^2}}=f\dfrac{M}{(r^2+a^2)^{\frac{3}{2}}}$.

find that the magnetic moments of the bars are got by multiplying the ordinates by 41·085378. Therefore, to get their magnetic moments *per gram*, we have to multiply the ordinates of IX and X by 0·3604, and those of XI by 0·3287.

The magnetising force is evaluated by the equation—

$$F=4\pi nC. \qquad (3.)$$

where n is the number of turns of wire in the coil per centimetre of its length, and C the current strength in c.g.s. units passing through it.

The abscissæ of the curves are scale divisions of the galvanometer, with its magnetometer at the platform division 32, or the readings at other divisions reduced to those of this division. Thus the abscissæ multiplied by $\frac{H}{10\times 32}$ represent the currents in absolute measure. The magnetising force F is therefore obtained from the curves by multiplying the abscissæ by $4\pi n\frac{H}{320}$, or $\pi n\frac{H}{80}$. In the first eight experiments, $n=45$, and $H=0·16$; thus we have 0·2828 as the factor for reducing the abscissæ of the curves I to VIII inclusive. The abscissæ of curves IX, X, and XI are reduced by the factor 0·224, n being 37, and H 0·16.

From a comparison of the curves Nos. I to IV inclusive, it will be seen that the "charcoal iron" has the highest magnetisability, and the "soft steel" the lowest, while that of the "soft Scotch iron" approaches very near to the former. With regard to retentiveness, the "charcoal iron" shows the least, and the "soft steel" the greatest.

Passing on to curves V to VIII inclusive, we find that the general effect of annealing the wires has been to lower their retentiveness, and to raise their magnetisability for all forces. In the "soft Scotch iron" wire little difference has been made; but in the others the effect is very marked, and is most noticeable in the "charcoal iron" wire with respect to its magnetisability, and in the case of the "soft steel" wire with respect to its retentiveness. Comparing the results of these eight experiments then, we find that the specimen which has the highest magnetisability, and at the same time the lowest retentiveness, is the annealed "charcoal iron" (7). Coming next to this specimen, in both respects we have the "charcoal iron" wire (3), and the annealed "common wire" (6), between which there is little or no difference. The "soft steel" wire is the lowest of all in respect of magnetisability, and highest in retentiveness. Annealing it, however (8), has had the effect of bringing it very close to the "common wire" (2).

As regards the bars, the second specimen of cast iron is greatly

inferior to the first in respect of magnetisability. The malleable iron bar exhibits a very much higher magnetisability than the cast iron bars; and its residual magnetisation was so low that it could not be observed with the same arrangement of apparatus.

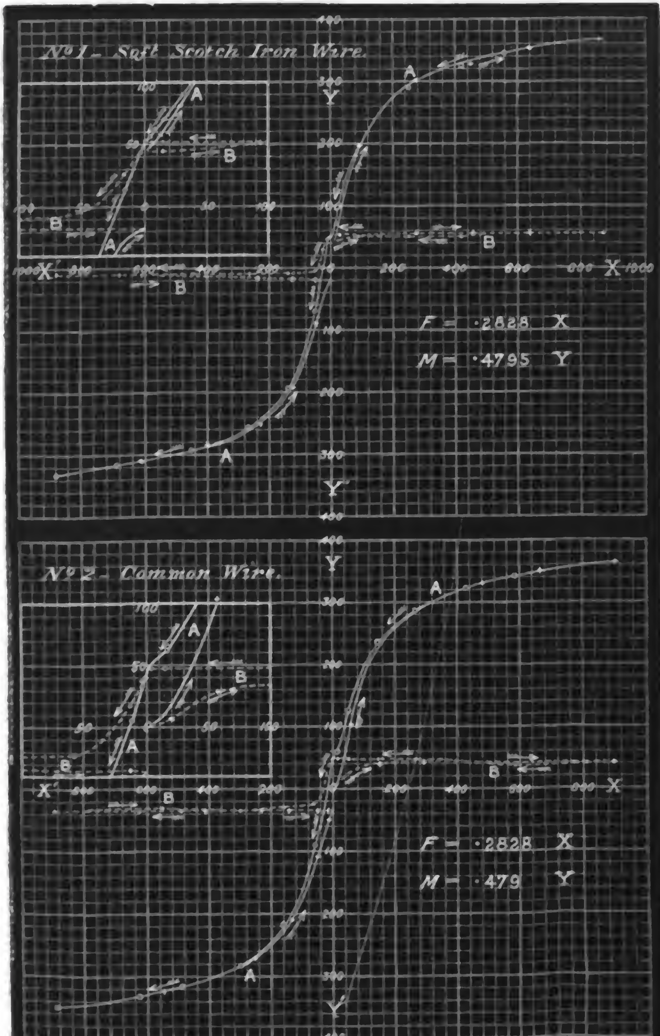
These are the main points in a comparison of the results of the experiments. A study of the curves, however, reveals many points of interest, one or two of which I may here indicate.

The curve beginning from the zero of magnetisation was not obtained in all the experiments, the wires having been previously magnetised in a preliminary test; but in those cases in which the smallest magnetising forces were employed, the curve of results is seen to be for a short distance concave towards OY.

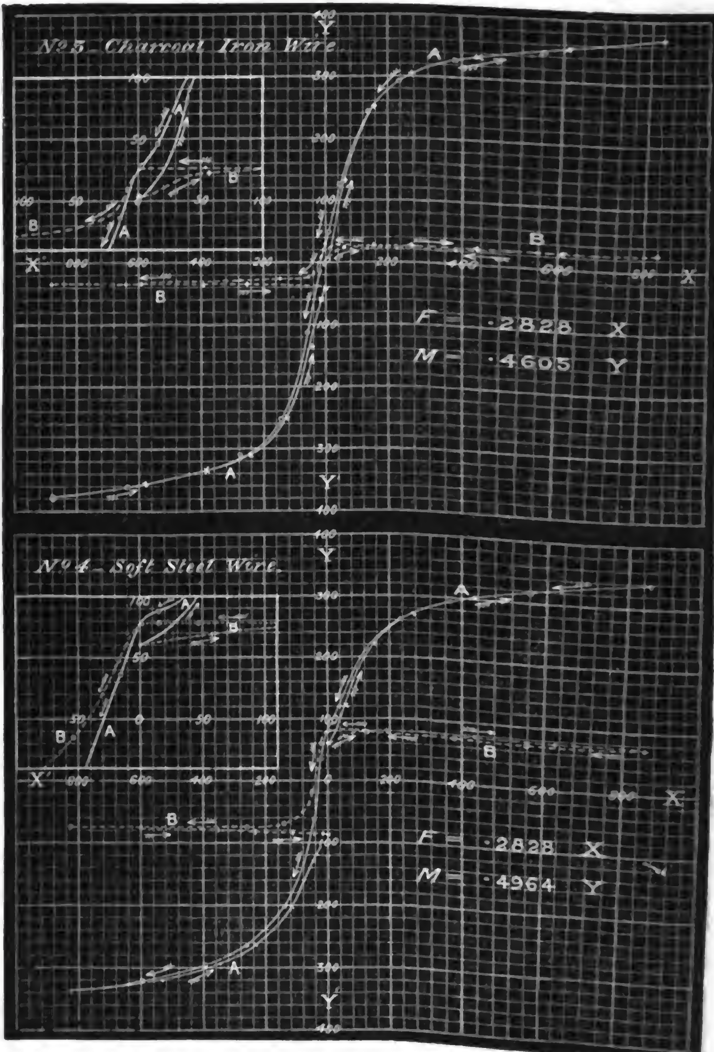
Returning again to the small magnetising forces after having proceeded to the limit, we find the curve first becoming concave towards OY, and then convex just before it crosses that line. On the negative side of OY, it remains concave for but a short distance, and is convex when it crosses the line of zero magnetisation, remaining so both in the direct and the return curves until we again near the zero of magnetising force, when it becomes concave for a short distance up to zero. To show these points clearly, the central portion on an enlarged scale has been affixed to each set of curves.

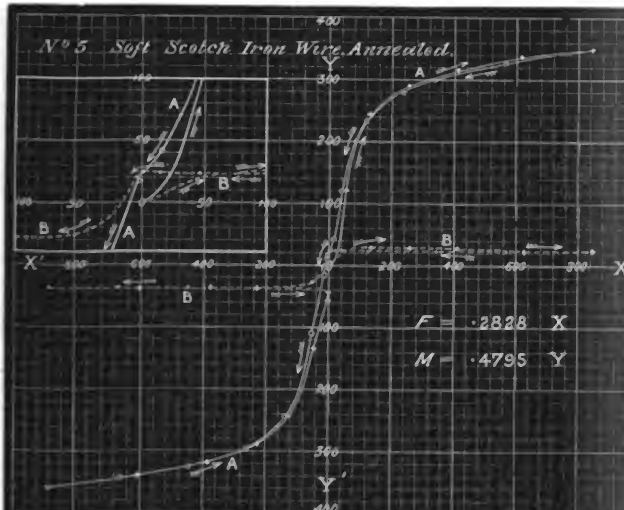
Turning to the curves for the residual magnetisation, an interesting point at once presents itself. This is a loop between the direct and return curves, more or less marked in most of the diagrams, but best seen in No. IV. A similar loop is seen in the curves of total magnetisation in Nos. V and VII, and there seems to be a tendency to form such a loop in all these curves. Regarding that part of the positive return curve which represents the effects of the small magnetising forces, we see that the residual magnetisation first begins to take a greater value, and then diminishes again just before the zero of magnetising force is reached.

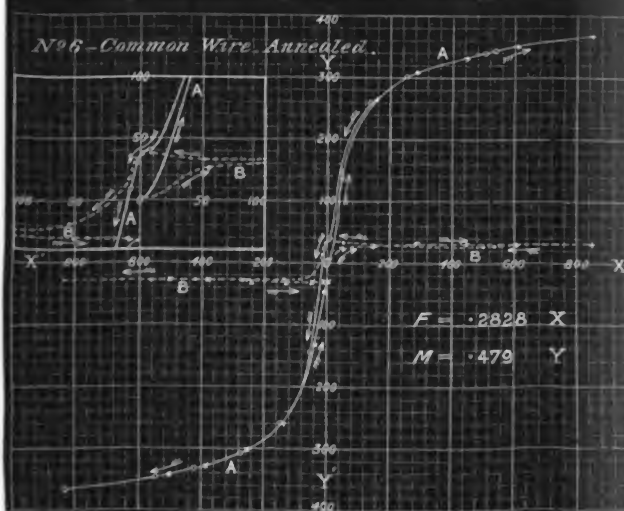
I shall defer any discussion of these anomalies until I have made a further observation of them under conditions more suitable for their special investigation.

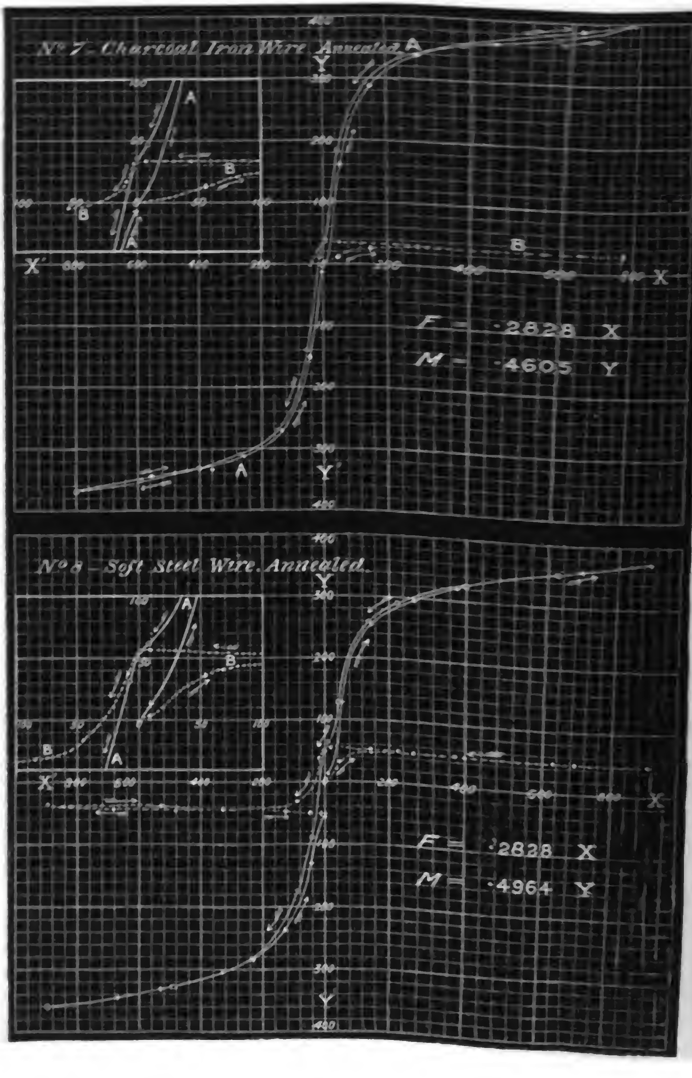


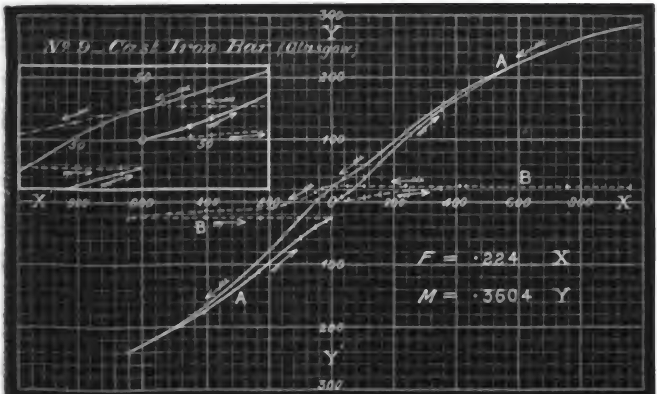
Nº 1 – Soft Scotch Iron Wire.
F = ·2828 X
M = ·4795 Y
Nº 2 – Common Wire.
F = ·2828 X
M = ·479 Y

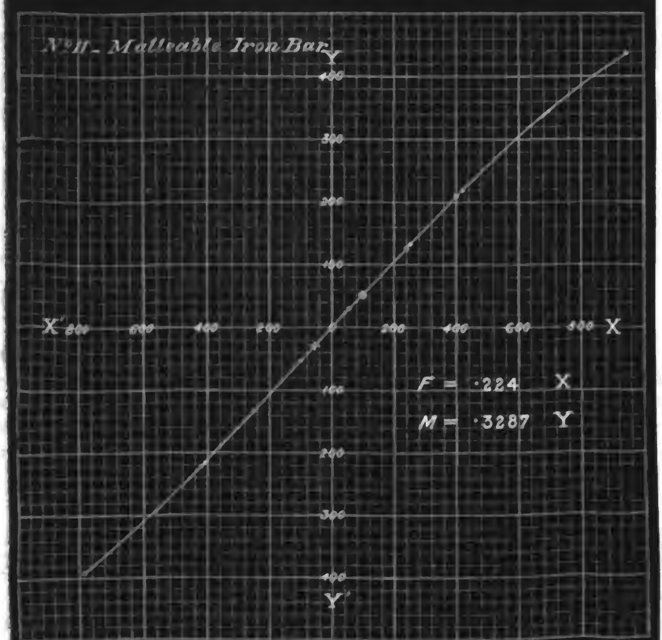

No. 3. Charcoal Iron Wire.
F = ·2828 X
M = ·4605 Y
No. 4. Soft Steel Wire.
F = ·2828 X
M = ·4964 Y

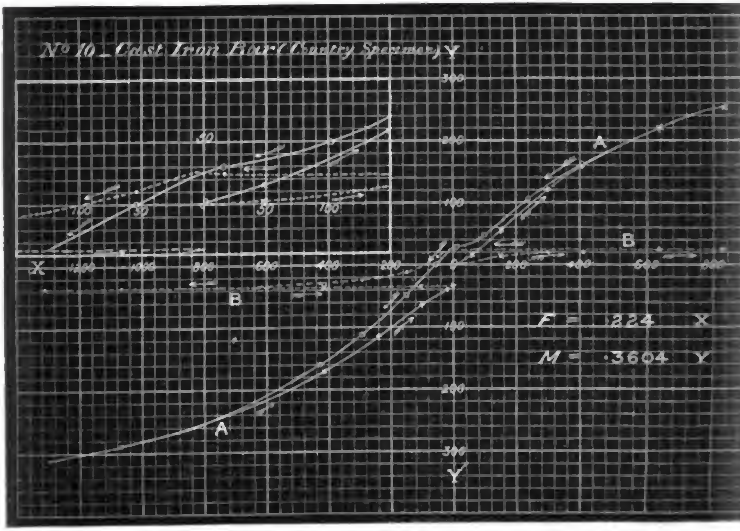

No 5. Soft Scotch Iron Wire. Annealed.
F = ·2828 X
M = ·4795 Y


No 6. Common Wire. Annealed.
F = ·2828 X
M = ·479 Y


No. 7. Charcoal Iron Wire Annealed.
F = ·2828 X
M = ·4605 Y
No. 8. Soft Steel Wire Annealed.
F = ·2828 X
M = ·4964 Y


Nº 9 _ Cast Iron Bar (Glasgow)
Y
A
B
X
X
B
A
F = ·224 X
M = ·3604 Y
Y'


Nº 11 _ Malleable Iron Bar
Y
X'
X
F = ·224 X
M = ·3287 Y
Y'



III. "On the Limited Hydration of Ammonium Carbamate." By H. J. H. Fenton, M.A., F.C.S., F.I.C., Demonstrator in Chemistry in the University of Cambridge. Communicated by Dr. Hugo Müller, F.R.S. Received November 19, 1885.

It occurred to me that a study of the action of water on ammonium carbamate, with reference to the influence of time, mass, and temperature, would be of interest as tending to throw light upon the laws which govern a chemical action of the simplest type in the liquid state —the action consisting of the direct union of two simpler molecules to form one more complex—

$$CO(NH_2)(ONH_4)+OH_2=CO(ONH_4)_2.$$

There are but few such actions which can be investigated, where all the substances are in the liquid state and all extraneous matter absent.

In a paper read before the Chemical Society in 1879,* I showed that ammonium carbamate when acted upon by sodium hypochlorite in

* "Chem. Soc. Jour.," 35, 12.

presence of sodium hydroxide, yields one-half of its nitrogen in the free state, the other half remaining in the form of sodium carbamate—

$$2CO(NH_2)(ONH_4)+3NaClO+2NaOH=2CO(NH_2)(ONa)+3NaCl +5OH_2+N_2.$$

Sodium hypobromite at once decomposes sodium carbamate, evolving its nitrogen in the free state—

$$2CO(NH_2)(ONa)+3NaBrO+2NaOH=2CO(ONa)_2+3NaBr +3H_2O+N_2.$$

This, in fact, may be claimed as a specific reaction for carbamates—no other substance yet investigated will yield nitrogen when treated with a hypobromite *after* the completed action of a hypochlorite. Urea evolves but half its nitrogen with a hypochlorite, in presence of caustic alkali, but the other half remains as a cyanate, which is not acted upon by a hypobromite.*

Based upon this reaction, then, we have a direct and simple method of determining the amount of carbamate existing in a solution at any given time.

Since, under the action of a hypochlorite, ammonium carbamate yields one-half of its nitrogen (*i.e.*, that present as ammonium), it is evident that any excess over and above this half which is evolved from its solution, is a measure of the water which has been assimilated converting the carbamate into carbonate.

For the sake of convenience we may express the hydration which occurs in terms of the ratio of the number of molecules of water assimilated to that of the molecules of ammonium carbamate taken. Let $V =$ the total volume of nitrogen contained in the ammonium carbamate taken, and $V_1 =$ that evolved by the action of a hypochlorite on its solution. Then the above ratio evidently $= \frac{2V_1 - V}{V}$ (since one atom of nitrogen evolved in excess of the half total corresponds to one molecule of water assimilated, and two atoms of nitrogen originally present represent one molecule of ammonium carbamate taken).

The ammonium carbamate used in these experiments was prepared by the direct union of carefully dried ammonia and carbon dioxide. The apparatus employed in the estimation of the evolved nitrogen was similar to that described in a former paper on the action of Hypochlorites in Urea.*

Experiments were undertaken with a view of investigating to what extent the action is influenced by (1) time, (2) mass, (3) temperature, and (4) to study the reverse action, namely, the dehydration of normal ammonium carbonate into ammonium carbamate.

* "Chem. Soc. Jour.," 33. 300.

I. *Influence of Time.*

Weighed quantities of ammonium carbamate were dissolved in water and the solutions made up to a definite volume. Measured portions were then withdrawn and examined at stated intervals by treatment with sodium hypochlorite and sodium hydroxide, care being taken to employ approximately the same quantities of reagents for each experiment of the series. The times were reckoned from the moments of complete solution of the salt.

6·2873 grams ammonium carbamate were dissolved in water—the solution made up to 100 c.c. and 5 c.c. (corresponding to 0·31437 gram of carbamate) taken for each experiment. Theory for total nitrogen = 90·20 c.c.

Minutes.	Intervals.	c.c. of Nitrogen (corrected).	Hydration.
5	1	54·16	0·2008
10	2	56·71	0·2575
20	4	60·37	0·3386
40	8	66·04	0·4644
60	12	68·60	0·5211
100	20	71·44	0·5842
120	24	72·08	0·5983
160	32	72·72	0·6125

After a further interval of about 24 hours, 72·28 c.c. of nitrogen were evolved. Similar experiments were made with a weaker solution, namely, one containing 4·8781 grams of ammonium carbamate in 250 c.c.; 10 c.c. were taken for each determination, corresponding to 0·1951 gram of carbamate, and to a total of 55·98 c.c. of nitrogen.

Minutes.	Intervals.	c.c. of Nitrogen. (corrected).	Hydration.
5	1	35·09	0·2536
10	2	38·72	0·3833
20	4	40·91	0·4615
40	8	43·20	0·5434
60	12	45·70	0·6327
120	24	47·47	0·6960
220	44	47·57	0·6995

After about 24 hours 47·96 c.c. of nitrogen were obtained.

It is evident from these results that the action, which proceeds rapidly at first, becomes progressively slower, and finally reaches a limit short of complete hydration.

The time required to reach a determinate state of hydration is evidently greater for a strong solution than a weak one, *i.e.*, decreases as the relative number of water molecules increases.

II. *Influence of Mass.*

8·3272 grams of ammonium carbamate were dissolved in water and the solution made up to 100 c.c. (Solution A.)

25 c.c. of solution A were made up to 50 c.c. (Solution B.)

25 c.c. of solution A were made up to 250 c.c. (Solution C.)

The relative strengths were therefore A : B : C : : 1 : $\frac{1}{2}$: $\frac{1}{10}$.

The solutions were set aside for four days in carefully stoppered flasks and under similar conditions. Volumes of each solution which corresponded to equal masses of ammonium carbamate were then withdrawn—namely, 5 c.c. of A, 10 c.c. of B, and 50 c.c. of C, representing 0·41636 gram of carbamate—and examined by the hypochlorite method in the usual way.

5 c.c. of	A gave	95·73	c.c. N (corr.).
10 ,,	B ,,	99·99	,,
50 ,,	C ,,	110·60	,,

In order to be certain that the equilibrium state had been arrived at, the same solutions were again examined after a further interval of two days, when

5 c.c. of	A gave	95·35	c.c. N (corr.).
10 ,,	B ,,	99·96	,,
50 ,,	C ,,	110·83	,,

showing that the limits had been reached in the former experiments. These numbers correspond to the hydrations—

A	0·5963
B	0·6735
C	0·8550

In order further to confirm these results, the residues from the last experiments, after the completed action of the hypochlorite, were treated with sodium hypobromite in order to estimate the nitrogen remaining as carbamate.

Residue from	A gave	22·02	c.c. N (corr.).
,, ,,	B ,,	17·50	,,
,, ,,	C ,,	8·01	,,

making the totals—

A	117·37	c.c. N (corr.).
B	117·46	,,
C	118·84	,,

theory for 0·41636 gram ammonium carbamate requiring 119·46 c.c. There is always a loss of about 8 per cent. of nitrogen in estimations

with hypobromite*: if this correction be applied the agreement will be still closer.

In order to facilitate the interpretation of the results, further experiments were made in the same direction, using mixtures of carbamate and water in simple ratios of their molecular weights.

				Mols. of carbamate.		Mols. of water.
(a)	0·3270 gram am. carb. with	15·09 grams water,	corresponding to	1	:	200
(b)	0·1836 ,, ,,	12·71 ,,	,,	1	:	300
(c)	0·1307 ,, ,,	12·06 ,,	,,	1	:	400
(d)	0·1042 ,, ,,	12·02 ,,	,,	1	:	500

After standing for some days under similar conditions the following results were obtained:—

	c.c. of N (corr.) obtained.	Theory for total nitrogen.	Hydration.
(a)	82·02	93·80	0·7487
(b)	47·29	52·66	0·7962
(c)	34·57	37·49	0·8447
(d)	28·62	29·89	0·9156

Another experiment was made in which the ratio of the molecular weights was nearly 1 : 1, namely, 0·2614 gram ammonium carbamate with 0·0632 gram water. After standing for two days, 49·52 c.c. nitrogen (corr.) were obtained—theory for total nitrogen requiring 75·0 c.c. The hydration was therefore 0·3187.

It follows from the above results that the hydration is a function of the number of water molecules present. So far the minimum hydration corresponds to the case in which the substances are present in about equal molecular proportions, and is in this respect analogous to the combination of iodine with hydrogen† and of phosphorus trichloride with chlorine.‡ From analogy it was to be expected that the hydration would again increase as the ratio of the ammonium carbamate molecules to those of water increased; but the experiment cannot be made under the same conditions as those above, since with any appreciable excess of the carbamate the water is insufficient to completely moisten the solid.

III. *Influence of Temperature.*

The above results may be taken as representing the phenomena which occur at about 20—22° C., from which the temperature varied but little throughout the experiments.

* Russell and West, "Chem. Soc. Jour.," 27, 749.

† Lemoine: "Équilibres Chimiques entre l'Hydrogène et l'Iode Gazeaux," "Ann. Chem. Phys." [5], xii, 145.

‡ Wurtz, "Comptes Rendus," lxxvi, 602.

In order to gain some idea as to the effect which temperature might have upon the action, two equal volumes of an arbitrary solution of ammonium carbamate were taken and kept in sealed vessels, one at the temperature of the room—about 15° C., and the other in melting ice. After about five hours the solutions were examined by the hypochlorite method, taking care to keep the apparatus in each case as nearly as possible at the respective temperatures.

Solution at 0° C.	gave 68·0 c.c. N	(corr.).
„ 15	„ 79·4	„

Similar pairs of experiments were made with another solution at stated intervals, equal volumes being measured out in the first instance and then subjected to the respective temperatures in sealed vessels.

After 30 minutes	solution at 0° gave 63·7 c.c.,	solution at 15° gave	76·2
„ 1 hour	„ „ 73·0	„ „	83·2
„ 2 hours	„ „ 74·0	„ „	88·9
„ 4 „	„ „ 76·3	„ „	90·6

The hydration is therefore, in all cases, less at the lower temperature.

The minimum hydration will probably occur at a sufficiently low temperature, and when the substances are present in equal molecular proportions. A mixture was made of 0·6199 gram ammonium carbamate with 0·1424 gram of water—corresponding almost exactly to equal molecular weights. This mixture was kept in a sealed tube placed in a block of ice for 18 hours, when it may be safely assumed that the equilibrium state was arrived at. It was then examined by the hypochlorite method (the apparatus being kept at about 0° C. throughout), and yielded 95·90 c.c. of nitrogen (corrected). Theory for total nitrogen requires 177·1 c.c., so that the hydration is represented by 0·0830.

Probably, at a sufficiently low temperature, the hydration would be practically *nil*, when the substances are present in molecular proportions—*i.e.*, ammonium carbamate and water would practically *not combine at all*—resembling the cases of phosphorus trichloride and chlorine, &c., at sufficiently *high* temperatures.

IV. *Dehydration of Normal Ammonium Carbonate.*

The salt was prepared by the method of Divers.* About 5—6 grams of it were dissolved in water, and the solution made up to 100 c.c. (A). Portions of this solution were diluted to $\frac{1}{2}$ (B) and $\frac{1}{10}$ (C). After two days—

* "Chem. Soc. Jour.," 23, 179.

5 c.c. of	A	gave	35·54	c.c. N (corr.)	
10 „	B	„	38·10	„	
50 „	C	„	40·85	„	

Equal volumes of another solution were examined at intervals, with the following results:—

After 5 minutes	67·94 c.c. N (corr.)	were evolved.
„ 40 „	66·73 „	„
„ about 3 hours	62·52 „	„
„ „ 24 „	62·28 „	„

These results indicate that normal ammonium carbonate undergoes dehydration into carbamate when in solution, and that the dehydration is greater as the relative number of water molecules is less.

The instability of normal ammonium carbonate, and the difficulty of obtaining it free from adhering impurities has, for the present, prevented me from making more extended observations on this part of the subject. It seems not unlikely, that if the same relative number of molecules could be started with, the same equilibrium state between carbamate, carbonate, and water, would be arrived at for the same temperature, whether ammonium carbamate or normal carbonate were initially taken—that is, that the curves of hydration and dehydration would meet at the same point. I hope before long to be able to make experiments in this direction.

Since there is this tendency for ammonium carbonate to become in part dehydrated in aqueous solution, and for the system to come to a state of equilibrium where the carbamate and carbonate co-exist, it seems probable that the hydrolysis of urea, under the action of ferments, &c., may be less simple than is usually represented—that the reaction, instead of being simply $CO(NH_2)_2+2H_2O=CO(ONH_4)_2$, may be of the following type:—

$$n CO(NH_2)_2+(2n-m)H_2O=m CO(NH_2)(ONH_4)+(n-m)CO(ONH_4)_2.$$

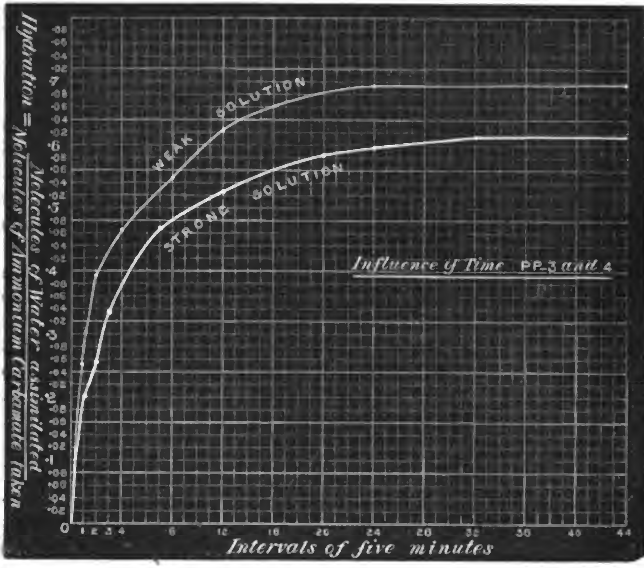
I propose to attack this subject by the same method as those employed above.

By means of the actions of sodium hypochlorite and hypobromite it is evidently possible and easy to estimate quantitatively *urea*, *carbamic acid*, and *ammonia*, when all are present in the same solution.

Take two equal portions of the solution to be examined, treat one with sodium hypobromite, and let the volume of nitrogen evolved (corrected for loss) $=V_1$.

Treat the other portion with sodium hypochlorite, and let $V_2=$ volume of nitrogen obtained.

Act upon the residue from this last experiment with sodium hypo-



bromite, and let V_3 represent the volume of nitrogen given off (corrected).

Let x = volume of nitrogen due to urea.
,, y = ,, ,, ammonia.
,, z = ,, ,, carbamic acid.

Then

(1.) $x+y+z=V_1$.

(2.) $\frac{x}{2}+y=V_2$.

(3.) $z=V_3$.

A preliminary trial of this method with a mixture of urea and ammonium carbamate gave very satisfactory results.

It appears to me that no really satisfactory method has hitherto been known by which carbamic acid could be detected and estimated in presence of urea and ammonia, so that the process indicated above may perhaps be of service in investigations bearing on the formation of urea in the animal body, and its origin, whether carbamic or otherwise.

IV. "On the Relation of the Reptiliferous Sandstone of Elgin to the Upper Old Red Sandstone." By Professor JOHN W. JUDD, F.R.S., Sec. G.S. Received November 19, 1885.

The question of the geological age of the yellow sandstones of the district lying to the north of the city of Elgin has been, as is well known, the subject of very animated discussions among geologists. Some have even gone so far as to assert that the evidence on the question, which has been adduced by palæontologists, is absolutely incapable of reconciliation with that relied upon by stratigraphists.

Until the discovery of fossils in the beds in question, it was quite natural to suppose that these white and yellow sandstones, which locally assume reddish tints, are part and parcel of the Upper Old Red Sandstone—a formation presenting somewhat similar mineral characters, and covering a considerable area in the district. This was the view which was taken by Murchison and Sedgwick, Dr. Malcolmson, Dr. Gordon, Mr. A. Robertson of Inverugie, Captain Brickenden, Mr. Patrick Duff, Mr. Martin, and indeed of all the geologists who at first studied the relations of the rocks in Morayshire.

But when fossils, which proved to be of great interest and importance both to the geologist and the biologist, were detected in this formation, a careful re-examination of the evidence relied upon by these geological pioneers was called for. The nature of these remarkable fossils was indeed calculated to awaken the gravest doubt as to the correctness of the commonly received opinions concerning the position in the geological series of the strata which yielded them.

In 1844 Mr. Patrick Duff obtained from the quarries at Lossiemouth, near Elgin, a cast of portion of the dermal covering of an organism of considerable size. Drawings of this fossil were sent by Mr. A. Robertson, of Inverugie, to Agassiz, and were by that naturalist described under the name of *Stagonolepis Robertsoni*.* Agassiz regarded the fossil as the impression of the scales of a ganoid fish allied to *Megalichthys* and *Glyptopomus*. Six years later, in 1850, Captain Brickenden obtained, from quarries in the same rock, at Cummingstown, a series of remarkable footprints, which were believed by most palæontologists to indicate the existence of reptiles at the time when the beds were deposited; and in the following year Mr. Patrick Duff obtained from the Spynie quarries the skeleton of a small Lacertilian, which was described by Mantell under the name of *Telerpeton Elginense*.† Shortly after this, many more footprints were detected

* "Monographie des Poissons Fossiles du Vieux Grès Rouge, &c." (1844–5), p. 139.

† "Quart. Journ. Geol. Soc.," vol. viii (1852), p. 97.

in the quarries about Cummingstown and Hopeman, and casts of the dermal armour of *Stagonolepis* were obtained from Findrassie, a little to the south-west of Spynie.

In 1858, Professor Huxley made the important announcement that *Stagonolepis* was not a fish, as supposed by Agassiz, but a Crocodilian; at the same time stating that he had recognized the remains of a third species of reptile among fossils sent to him by Dr. Gordon from Lossiemouth; this new reptile subsequently received the name of *Hyperodapedon Gordoni.**

In 1867 the same author was able to demonstrate from fresh and better preserved specimens which had been discovered at Lossiemouth, that *Telerpeton* was not only a true Lizard, but one of a very specialised type;† and two years later he gave a full description of the structure and zoological position of *Hyperodapedon*, showing it to have been a Lacertilian having affinities with the recent *Sphenodon* and the Triassic *Rhynchosaurus;* he was also able to announce the discovery of remains of the same genus in the Keuper of Warwickshire and Devonshire, and in the Maledi (Trias) beds of India.‡

The year 1877 was marked by the publication of Professor Huxley's Monograph on *Stagonolepis*, in which he referred to an interesting cast of a jaw from the Findrassie quarry, to which he gave the name of *Dasygnathus longidens.* This rather obscure fossil, it was shown, might possibly be referable to the fishes or Labyrinthodonts, though it presents some points of resemblance with the Dinosaurs.§

In the year 1884 I saw in the Elgin Museum the cast of a skeleton which had recently been obtained from a quarry newly opened near Elgin, and to be more particularly referred to in the sequel. This fossil appeared to me to be so different from all the remains hitherto found in the formation, that I obtained an impression of it and submitted it to Professor Huxley, who recognised in it certain characters distinctive of the Dinosauria. From the same quarry a skeleton apparently belonging to another lizard, distinct alike from *Telerpeton* and *Hyperodapedon*, with portions of the skeleton of the last-mentioned genus, were also obtained.

Returning to Elgin in the autumn of the present year, I was told by my friend Dr. Gordon that another reptilian specimen, including the skull and some other parts of the skeleton, had been found in the same quarry. On examining this specimen I at once saw that it exhibited the characteristic features of *Dicynodon*, and my opinion on the subject was confirmed by my friend Dr. Traquair, F.R.S., of

* "Quart. Journ. Geol. Soc.," vol. xv (1859), p. 440.

† *Ibid.*, vol. xxiii (1867), p. 77.

‡ *Ibid.*, vol. xxv (1869), p. 138.

§ "Memoirs of the Geological Survey of the United Kingdom," Monograph III (1877).

Edinburgh, who, at my request, proceeded to examine the specimen. A second example of the same genus has since been discovered, and I trust that ere long a full description of this interesting addition to our British fossils will be given by Dr. Traquair.

In addition to these facts, I may add that casts of teeth, undistinguishable from those of *Ceratodus*, were some time ago obtained from the Spynie quarries.

The present state of the palæontological evidence concerning the age of the beds then is as follows. The strata have yielded the remains of no less than *four orders* of reptiles, all of them belonging to forms very different to any which have been found in Palæozoic rocks. The Lacertilia are represented by *Telerpeton*, *Hyperodapedon*, and an undescribed form; Crocodilia by *Stagonolepis;* Dinosauria by an undescribed skeleton and possibly by *Dasygnathus;* and Dicynodontia by two individuals belonging to the type genus of the order. In addition to these we have a great number of footprints differing so greatly in form and size that they must probably have been made by creatures of very different proportions and organisation. Professor Huxley in his later researches on the subjects of these footprints* points out the necessity of the greatest caution in any attempts to correlate them with either of the reptiles whose skeletons have been found in the same formation; indeed, he hesitates as to whether the most perfectly preserved of them should be referred to Amphibians or to representatives of one or other of the orders of Reptilia. Among the less perfect of the markings in these and other rocks of the Elgin district, there are not a few concerning which I have serious doubts whether they are to be ascribed to vertebrate animals at all.

It will be seen from this summary that the palæontological evidence in favour of the Triassic age of the Elgin sandstones is now absolutely overwhelming. Besides the remains of *Hyperodapedon* and *Dicynodon*, genera which appear to be confined to Triassic strata, in districts so widely separated as South Africa, India, the Ural Mountains, and the British Islands, we have *Stagonolepis*, a crocodile with Mesozoic affinities, the highly organised *Telerpeton*, and Dinosaurs; the last mentioned having never been found in any rocks older than Trias. *Ceratodus*, too, has usually been regarded as having commenced in the Trias, though it must be admitted that difficulty may exist in separating the cast found at Spynie from *Ctenodus*, which occurs in the Carboniferous, or *Dipterus*, which occurs in the Devonian.

There are certain facts concerning the distribution of these fossils in the formation where they occur, to which it may be instructive to refer as bringing out into strong relief the imperfection of the geological record. The footprints, which are so abundant at Cummingstown and Hopeman, would appear to have been seldom, if ever,

* "Mem. Geol. Survey," Monograph III, pp. 45–51.

found in the quarries where the bones and scutes occur. All the specimens of these bones and scutes which have been obtained at Lossiemouth seem to have occurred in a single course of rock at the bottom of the quarries, where the useful building stone ceases. All the specimens obtained from this locality seem to belong to *Stagonolepis*, *Hyperodapedon*, and *Telerpeton*. In the new quarry near Elgin, however, neither *Stagonolepis* nor *Telerpeton* has been found, but Dinosaurs and Dicynodonts occur.

Let us now inquire what is the nature of the stratigraphical evidence which has been regarded as opposed to the palæontological arguments in favour of the Triassic age of this formation. At the outset it is necessary to bear in mind two very important circumstances. *First.* The exposures of the Reptiliferous Sandstone and of the Upper Old Red in the district are more or less isolated, the greater part of the country being thickly covered by drift and other superficial deposits. *Secondly.* The whole of the rocks in the district exhibit evidence of having undergone great disturbance; this is shown by their steep inclinations, and by the foldings and fractures which can often be recognised in the quarries opened in them.

The Reptiliferous Sandstone makes its appearance at the surface in two parallel ridges, ranging from north-east to south-west for a distance of about nine miles. The most northerly of these ridges extends from Brandenburgh to Burghead. Although the rocks are well exhibited both in sea-cliffs and in reefs on the shore, the only fossils obtained from them are the footprints of the Cummingstown and Hopeman quarries, near the south-western extremity of the ridge, and the remains of *Stagonolepis*, *Telerpeton*, and *Hyperodapedon*, found in a single bed at Lossiemouth, at its north-eastern end. A tract of about three miles wide, thickly covered by superficial deposits, completely isolates the northern or coast ridge from the southern one, which is known as the Quarrywood ridge. In this Quarrywood ridge the Reptiliferous Sandstone is only found along its northern face for a distance of about three miles. The southern slope of the ridge is composed of the ordinary rocks of the Upper Old Red Sandstone, containing *Holoptychius nobilissimus* Ag., with species of *Glyptopomus* and *Pterichthys*. There is no evidence of the occurrence of Triassic strata, either along the southern slopes of the Quarrywood ridge or in the district lying still further south about the city of Elgin. The localities in which the sandstone containing reptiles has been found along the northern slope of the Quarrywood ridge are as follows:—At Spynie, which may be regarded as a north-eastern prolongation of the Quarrywood ridge, the deep quarries have yielded *Telerpeton*, *Hyperodapedon*, and *Ceratodus*. At Findrassie Wood, a mile and a half further to the south-west, a quarry now abandoned, has yielded *Staganolepis* and *Dasygnathus*. Lastly, the quarry near the top of the ridge, above

New Spynie Church, and a mile and a half still further to the south-west than Findrassie, has yielded *Hyperodapedon* and another lizard with a Dinosaur and a Dicynodont.

The difference in mineral characters between the Triassic Sandstone on the northern side of the Quarrywood ridge and the Upper Old Red Sandstone on its southern face is certainly not a very striking or well-marked one. But this is a circumstance at which no geologist who is in the habit of studying the Old and the New Red Sandstone will be surprised. Nevertheless, a careful study of the two sets of rocks shows that there are appreciable differences between them, and, as a matter of fact, practised observers like Dr. Gordon seldom find any real difficulty in pronouncing at a glance whether any particular mass of building-stone belongs to the "reptiliferous" or the "holoptychian" formation. It must be admitted, however, that occasionally the pale-pink Old-Red rock assumes a nearly white colour, while the white or yellow "reptiliferous" rocks locally acquire reddish tints, undistinguishable from those of the "holoptychian" sandstone.

In both the coast ridge and the Quarrywood ridge, as was well pointed out by Dr. Gordon, the Reptiliferous Sandstone is seen to be covered by a very peculiar and easily-recognisable deposit, known as the "Cherty rock of Stotfield." It has been frequently suggested that the preservation of these two sandstone ridges, and thus of the whole peninsula between Burghead Bay and Spey Bay, was in all probability due to the presence of this remarkable rock, which offers such resistance to the ordinary agents of denudation.* The rock consists of a more or less intimate admixture of siliceous and calcareous materials, including also crystallised patches of galena, blende, and pyrites; it has yielded no trace of organic remains. Sir Roderick Murchison compared the "Cherty rock of Stotfield" with the Cornstones of the Old-Red rocks, with which, however, it has but little in common; and some confusion appears to have arisen from bands of true Cornstone, which occur in Upper Old Red Sandstone to the south of Elgin, having been taken for the Cherty rock of the Trias.

Professor Harkness, in 1864, was able to show that the positions in which the Cherty rock and the Reptiliferous Sandstone occur in the neighbourhood of Elgin are such as can only be explained by the existence of great faults. At a later date, I showed how numerous are the indications of disturbance in the district—evidence of tilting of the beds, of actual contortion, and of fracture occurring in many of the quarries. In the New Bishopmill Quarries, for example, the effects of a fault, in throwing side by side beds of valuable freestone and other sandstones unsuitable for building purposes, is very clearly seen, and similar evidence is found all over the area where these beds occur. On the north of the coast-ridge I have shown that beds of

* "Quart. Journ. Geol. Soc.," vol. xx (1864), p. 424.

Inferior Oolite are seen faulted against the Trias of Stotfield,* and the same is probably the case also at Burghead. In the great "Scars" or reefs which lie off this coast red sandstones are seen, and I have been assured that scales of *Holoptychius* occur in them. If this be true, then the whole of the Mesozoic strata, forming the peninsula between Burghead Bay and Spey Bay, consists of rocks which are actually let down by trough-faults and synclinal folds into the midst of a tract of the Upper Old Red Sandstone. The presence of such great lines of dislocation is unquestionable, and in the paper referred to I have endeavoured by means of dotted lines to indicate the approximate position of some of them. It must be remembered, however, that in a country so deeply covered by drift as Northern Morayshire, the working out of the relations of the rock-masses by tracing their outcrops at the surface is an almost hopeless task.

As throwing an entirely new light on the age and relations of the Reptiliferous Sandstone of Elgin, I was able in the year 1873 to show that strata identical in character with that deposit and with the Cherty rock of Stotfield occur on the northern, as well as on the southern side of the Moray Firth. At Dunrobin, in Sutherland, the yellow sandstones are seen covered by the Cherty rock, and this in turn is overlain in apparently conformable sequence by the various members of the Lias and Oolite. The whole of the Mesozoic strata of Sutherland are seen to be thrown by a great fault against the Lower Old Red Sandstone and the Crystalline rocks of the Highlands.

Although it is certain, however, that some of the cases of juxtaposition between the Old Red and the Triassic strata must be due to faulting, yet there are reasons for believing that the latter strata lie directly and unconformably upon the former. But, as was remarked by Dr. Gordon in 1877, "the district is so covered by drift that no junction of the Holoptychian and the Reptiliferous strata has been laid bare."

It was therefore with the greatest interest that in the summer of 1884 I learned from that veteran geologist, whose important services to science have extended over a period of more than half a century, that the bones of reptiles had at last been detected in the same quarry with the remains of *Holoptychius*. On repairing to Elgin I received the greatest assistance in investigating the matter from Mr. J. Gordon Phillips, the intelligent and energetic curator of the Elgin Museum.

It appears that about the beginning of 1882 an old stone-pit, known as "the Millstone Quarry," and situated near the summit of the Quarrywood ridge, immediately above the church of New Spynie, had been reopened, and extensive excavations had since been carried on there. The beds here present a somewhat similar character to those

* "Quart. Journ. Geol. Soc.," vol. xxix (1873), p. 128, &c.

of Lossiemouth and Spynie, consisting of white or pale-yellow sandstone, containing occasional black particles composed of iron and manganese oxides. The sandstones of this pit are generally much coarser in grain than those at Lossiemouth and Spynie, and they sometimes even pass into a grit. In some of the beds, particles of felspar occur in such profusion as almost to cause the rock to assume the appearance of an "arkose." That these rocks really belong to the Reptiliferous Sandstone has been confirmed by the finding, up to the present time, of no less than six skeletons of reptiles, and by the total absence from them of the "Old-Red" fish-remains.

The Reptiliferous Sandstone, both of the coast ridge and of the Quarrywood ridge, not unfrequently contains scattered pebbles of quartz; but at the "Cutties' Hillock Quarry," as the new pit is now called, this feature is more strikingly exhibited, and such quartz-pebbles become very abundant, especially in some of the lower beds. As the excavations were carried downwards, indeed, the coarse sandstone was seen passing into a conglomerate, called by the workmen the "pebbly-post." This bed of conglomerate was found to be from 3 feet 6 inches to 4 feet thick, and, it being considered desirable to determine if other courses of freestone fit for building purposes underlie the "pebbly-post," a trial-shaft was opened at the bottom of the quarry.

It was discovered in this way that the "pebbly-post," which in its lower portion becomes more perfectly conglomeratic, and contains pebbles of white and purple quartz up to the size of the fist, rests on beds of pink and red sandstones, very finely laminated, and exhibiting evidence of much false-bedding. These beds are strikingly different in character from the coarse-grained, white sandstones lying above the "pebbly-post," in which the bedding is usually indistinct and imperfect. The stone lying below the conglomerate was found to be unsuited for building purposes, and the trial-shaft, after being carried to the depth of 13 feet in the bottom-rock, was abandoned; very fortunately, however, the last blast which was fired in it revealed a remarkably fine specimen of *Holoptychius*, which has been identified by Dr. Traquair as *H. nobilissimus*, and is now in the Elgin Museum.

It unfortunately happened that no careful geological study was made of the beds exposed in the trial-shaft at the time when it was open; but I was able, with the assistance of Mr. J. Gordon Phillips, and of Mr. Watts, the very intelligent lessee of the quarry, aided by an inspection of the materials thrown out, to substantiate the above facts and to add the following details:—

The red and finely-laminated sandstones of the Upper Old Red Sandstone are directly overlain by the bed of quartzose conglomerate. This latter bed, which is from 3 feet 6 inches to 4 feet in thickness, contains fewer and smaller pebbles in its upper part, and thus

graduates insensibly into the coarse Reptiliferous Sandstone, which forms a number of courses, each from 3 feet to 4 feet thick, and are exposed to a depth of over 20 feet. The Reptiliferous Sandstone in this pit section exhibits evidence of considerable disturbance; its beds dip to the north-east at an angle of about 15°, while, at one part of the pit, there are indications, in great slickensided surfaces and a slight displacement of the beds, of a small fault. In these upper sandstones remains of a least six reptiles have up to the present time been discovered, five of them occurring in one course of stone, while the remaining one came from the bed immediately below. The forms represented in this pit are *Hyperodapedon*, and another lizard, *Dicynodon*, and a Dinosaur.

The characters of the sandstones above the bed of conglomerate are very distinct from those of the sandstones below it. The former are very fine grained and have their lamination very strongly pronounced, exhibiting much false-bedding; while the latter are usually much coarser and seldom show any trace of stratification. The colours, too, are very distinctive, but this is a character upon which it would be unwise to place much reliance. Examined in thin sections, under the microscope, I found that the two sandstones present well-marked and constant differences.

These facts all point to the conclusion that the Reptiliferous Sandstone of Elgin passes downwards into a bed of conglomerate, which rests unconformably upon the strata of the Upper Old Red Sandstone.

During a visit to Sutherland last year, I also obtained evidence that a precisely similar relation in all probability exists between the Triassic rocks and the Upper Old Red Sandstone on the northern side of the Murray Firth.

Some years ago Dr. Joass of Golspie found remains of *Holoptychius* in the sandstones which crop out in reefs on the shore at some distance southward from that place. Between these reefs of Upper Old Red Sandstone and those of Dunrobin, where I was able in 1873 to identify the Reptiliferous Sandstone and the Cherty rock overlying it, the rocks are wholly concealed. But Dr. Joass showed me masses of a conglomerate which are frequently thrown up by the waves on the Golspie shore, containing yellow and purple quartz-pebbles, and identical in character with the rock of the "pebbly-post" in the Cutties, Hillock Quarry near Elgin. There can be little doubt that the bed from which these fragments are derived lies between the Trias and the Upper Old Red Sandstone of the Sutherland coast.

The Royal Society long ago testified its sense of the importance of determining the age and relations of the remarkable strata of Elgin, by appointing a Committee and making a grant from the Donation Fund to aid in securing new specimens of the fossils. Seeing, then, that an opportunity offered itself for determining the exact relations

of the Reptiliferous to the Holoptychian beds, I preferred a request to the Council of this Society for a grant to be applied in excavations directed to uncovering the line of junction between the two beds.

My request having been acceded to, the kind intervention of Dr. Gordon obtained for me the permission of Thomas Yool, Esq., of Leuchars, the factor of the Earl of Fife, on whose property the quarry is situated, for carrying out the necessary work. The Messrs. A. and W. Watt, the lessees of the quarry, not only rendered much valuable advice and assistance, but kindly undertook the personal superintendence of the necessary operations. In making a careful examination of the pit, after it had been opened, I had the great advantage of the aid and judicious counsel of Professor T. G. Bonney, F.R.S., President of the Geological Society.

We were able to observe that, while the conglomerate of the "pebbly-post" graduates insensibly into the overlying Reptiliferous Sandstone, it is sharply divided from the red sandstones below. It was unfortunately found that, owing to the imperfect bedding of the upper series and the prevalence of oblique lamination in the lower one, it was impossible to obtain decisive evidence of a discordance of dip between them. But the line of junction between the two sets of strata, which was exposed for a distance of 10 feet only, showed every appearance of being an eroded one. We came to the conclusion that while the upper series having the "pebbly-post" for its base, is certainly perfectly distinct from the lower one, there can scarcely be the smallest doubt that the former rests uncomformably upon the latter; in other words, the evidence points to the conclusion that during the vast periods of the Carbiniferous and Permian, the Upper Old Red Sandstone of the Elgin area was upheaved and denuded, and that subsequently the Upper Trias beds were deposited unconformably upon the eroded surface.

As the question of the age and relations of these interesting rocks may now be considered as definitely settled, it may be well to give a brief *résumé* of what is known concerning the interesting patches of Triassic strata in the east of Scotland, from which such important palæontological treasures have been derived.

There is reason for believing that the Trias of this district does not exceed 200 to 300 feet in thickness. At present it is known to occur only in the coast ridge, 9 miles long and about 1 mile broad, and on the northern slope of the Quarrywood ridge for a distance of about 3 miles. Outlying patches, like the Boar Rock in Spey Bay, show that its superficial extent has been greatly reduced by denudation and the deposit of drift upon it. Similar beds covering only a very small area however, make their appearance on the northern side of the Moray Firth, between Dunrobin and Golspie.

The lowest member of the formation consists of quartzose con-

glomerate, very similar to that which occurs in the Poikilitic of the west coast of Scotland, and of many parts of England. This, by the gradual diminution in the size and number of the pebbles, passes occasionally up insensibly into a coarse grit, containing scattered quartz pebbles, and, finally, into the very fine-grained white or yellow sandstone which constitutes the bulk of the formation.

In the whole of this deposit, organic remains are very sparsely distributed. The extensive sea-cliffs and shore-reefs of the coast ridge have not yielded a single specimen, nor have any fossils been found in similar situations in Sutherland. Many of the largely worked quarries have proved equally barren. Several quarries in immediate proximity to one another have, however, yielded footprints, and a single band of soft rock in the Lossiemouth Quarries, situated at the depth of about 100 feet from the top of the sandstone series, has yielded many remains of *Stagonolepis*, *Hyperodapedon*, and *Telerpeton*. It is not improbable that it is a bed on about the same horizon which has yielded *Telerpeton*, *Hyperodapedon*, and *Ceratodus* at Spynie, and *Stagonolepis* and *Dasygnathus* at Findrassie. The new quarry at Cutties' Hillock, however, is certainly opened in beds *lower* in the series, and, indeed, near its base. The only form up to the present found in this lower division which is common to it and the higher strata is *Hyperodapedon*, but the lower beds have also yielded a Dinosaur, a Dicynodont, and a new species of Lacertian. There are good grounds for anticipating further important discoveries in this part of the series, as large quantities of stone are now being taken from the quarry in which it is exposed.

The beds of the Reptiliferous Sandstone are often seen to be traversed vertically by masses of a hard quartzite-like rock, which are known to the quarrymen as "keys." Such masses are seen rising through the sandstones in the sea-cliffs, and in many of the quarries, where, being unfit for building purposes, they are either avoided in the quarrying operations, or are broken up to serve as road-metal. A key of this kind is present in the quarry at Cutties' Hillock. Microscopic examination of these quartzite-like masses shows them to consist of the ordinary white or yellow sandstone, in which silica has been deposited in the form of quartz upon and between the individual grains. As in the case of the "crystallised sandstones," and many quartzites, the secondary silica is in crystallographic continuity with the quartz grains on which it is deposited; this is clearly shown when thin sections of the rock are examined by polarised light, the orientations of the original and the secondary quartz, as exhibited by their optical characters, being thereby rendered manifest.

Overlying the Reptiliferous Sandstone is the very remarkable calcareous and siliceous rock, known as the "Cherty rock of Stotfield," the peculiar characters of which have been already indicated. At

present this member of the series is only seen as a number of isolated patches, and we have no evidence as to whether it ever constituted a continuous deposit. Its thickness is never great, and probably in no case exceeds 30 feet.

The "Cherty rock of Stotfield" which has afforded no traces of organic remains, even when studied under the microscope, is evidently a chemical and not an organic deposit. Its appearance and characters, indeed, strongly suggested that, like very similar deposits in Hungary, it may have been formed by geysers, an idea which was entertained by Sir Charles Lyell. If this be so, it is impossible to avoid entertaining the suggestion that the formation of the keys may have been due to the rise of heated water containing silica in solution along the joint planes of sandstones below. Some support is afforded to this suggestion by the fact that where, as at Stotfield, the Cherty rock is largely developed, there the quartzite "keys" are particularly numerous in the underlying sandstones.

It may be of some interest to add that the Trias of the district of Scania in Southern Sweden contains rocks quite undistinguishable in their mineral characters from the Pebbly Conglomerate, the Reptiliferous Sandstone, and the Cherty rock of the Trias of Eastern Scotland.

V. "Experimental Researches in Cerebral Physiology. II. On the Muscular Contractions which are evoked by Excitation of the Motor Tract." By V. A. Horsley, M.B., B.S., Professor Superintendent of the Brown Institution and Assistant Professor of Pathology in University College, London, and E. A. Schäfer, F.R.S., Jodrell Professor of Physiology in University College. Received December 1, 1885.

The following note gives the results of a large number of experiments which we have undertaken, in order to determine the character of the muscular contractions which result from excitation of the several parts of the motor tract, especially with reference to the rhythm with which the skeletal muscles respond to such excitation.

For the purpose of our experiments we may consider the motor tract under four heads, viz.:—1. Its commencement in the nerve-cells of the cerebral cortex. 2. The connexion of these cells with the lower nerve-centres by the nerve-fibres in the corona radiata. 3. Its continuation along the medulla oblongata and medulla spinalis (including the nerve-cells of those structures). 4. Its peripheral continuation along the motor nerves.

Methods.—Our method of proceeding has been to excite these several parts in succession, and record the contractions of one of the limb muscles upon a moving blackened surface, either by directly connecting the tendon with the lever of a myograph, or by Marey's method of transmission by tambours and indiarubber tubing, the time being simultaneously recorded upon the moving surface by a clock marking seconds. Usually the rate and duration of the excitation were also recorded by a small electromagnet. Besides the contractions resulting from electrical excitation, we have frequently obtained an accidental record upon the moving surface of spontaneous or voluntary contractions of the muscle the responses of which to electrical excitation of the cortex cerebri we were preparing to record, and we have thus been able to compare these records of voluntary contractions in animals both with the results of electrical excitation of the several parts of the motor tract in the same animals, and with records of voluntary contractions in the human subject. We have also studied in the same way the epileptoid contractions which are often found to follow a period of electrical excitation of the cortex cerebri in animals, and have compared these epileptoid contractions with numerous others recorded by one of us from cases of true epilepsy and other affections of the nervous system (in man and animals) accompanied by rhythmic muscular movements.

Results.—It was somewhat vaguely stated by Franck and Pitres,* and has generally been admitted by other authors, that so far as regards the rhythm of muscular response, the result of exciting either the cerebral cortex or any other part of the motor tract is precisely the same as that which is well known to be the case with the excitation of the motor nerve, namely, that for all rates of excitation the rhythm of muscular response is identical with the rhythm of excitation. Our experiments on the contrary show that this statement only holds good for low rates of excitation up to about ten or twelve per second, but that for all higher rates of excitation of the cortex cerebri, corona radiata, or medulla spinalis the muscular response does not vary with the rate of excitation, but maintains a constant rhythm which is independent of the excitation rate and approximates to ten per second.

The muscle-curves which we have obtained from different mammals as the result of successive excitation of the cortex cerebri, corona radiata after removal of the superjacent cortex, and of the cervical cord after section below the medulla oblongata, are very similar to one another, and exhibit along their course, both at the commencement and during the whole extent of contraction of the muscle, small but distinct undulations following one another at the rate of about ten

* "Travaux du Laboratoire de Marey," iv, 1879, pp. 412–447. See also a paper by the same author in the "Arch. de Physiologie," Nos. 1 and 2, 1885.

per second, with very considerable regularity, although in a few instances the rhythm may be a little slower or faster than these (eight to thirteen per second are the extreme variations recorded). These undulations have the same rhythm and character whatever the rate of excitation (unless this be allowed to fall below about ten per second). Moreover, precisely similar undulations are always visible upon the myographic curve of all voluntary or spontaneous contractions (including reflex contractions) both in the lower animals and in man.

It is further noted that in the record of the contractions of epilepsy there can frequently be seen marked upon the larger curves, produced by the relatively slow clonic spasms, smaller undulations succeeding one another with a rhythm of eight or ten per second. In some cases the clonic contractions themselves may attain this rate, but they are then always simple and without any indications of smaller waves.

In a very few instances out of a very large number of experiments there occurred upon the tracings obtained as the result of rapid excitation of the cortex cerebri, corona radiata, and medulla spinalis, besides the usual well marked undulations of the rate of about ten per second, other very minute waves upon these undulations corresponding in rhythm with the rate of excitation. These were the only occasions in which we have obtained results at all similar to those mentioned by Franck and Pitres.

The accompanying tracings will serve to indicate the general nature of our results. Tracings A, B, C, and D are all taken from the same animal (dog). Tracing A shows the myographic curve obtained during excitation of the cortex cerebri (sigmoid gyrus), as well as the succeeding epilepsy. Tracing B was that obtained on excitation of the subjacent corona. Tracing C resulted from excitation of the cut spinal cord. Tracing D from excitation of the motor nerve. The excitation was produced by varying the current through the primary coil of a sliding inductorium by a metallic reed vibrating thirty times per second in the case of A, C, and D, and forty per second in the case of B. The vertical lines mark seconds. E is the tracing of a voluntary muscular contraction in man (opponens pollicis).

Conclusions.—The main conclusions to be drawn from the results of our experiments appear to be these:—1. That the normal rate of discharge of nervous impulses from the motor nerve-cells of the spinal cord along the motor nerve-fibres is approximately ten per second.* 2. That in the case of nervous impulses reaching these

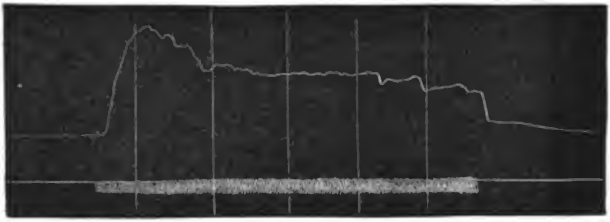
* This conclusion is supported by the fact that the rhythm of a clonus (*e.g.*, ankle-clonus) depending upon the activity of the spinal cord is also about 8 or 10 per second, and that the rhythm of strychnine-tetanus in the frog, as indicated by the electrical variations of the muscle-current, has about the same rate (Lovén).

a
b

A. Tracing obtained from the extensor longus digitorum of the fore-arm of a dog as the result of rapid excitation of the cortex cerebri. *a*, myographic curve; *b*, chronograph record of the period and rate of excitation. The ordinates mark seconds.

a
b

B. Tracing obtained as the result of excitation of the subjacent corona radiata after removal of the part of the cortex cerebri which was previously excited.



C. Tracing obtained from the same muscle as in A and B as the result of excitation of the distal end of the cut spinal cord.

D. Shows the result of rapid excitation of the motor nerve of the same muscle.

E. Tracing of a voluntary contraction of the opponens pollicis (man). The ordinates indicate seconds, as before.

nerve-cells in more rapid succession than about ten per second, a process of summation occurs within the nerve-cells, so that the rate of discharge remains about the same in all cases. 3. That the nervous impulses which produce a voluntary contraction also traverse the motor nerve-fibres at about the same rate. There is, however, no distinct evidence to show whether this rhythm of the volitional impulses is generated in the cells of the cerebral cortex, or in the cells of the lower nerve-centres. 4. That the slower rhythm which is often exhibited in epileptoid contractions is the result of a further summation, but there is no distinct evidence to show where this occurs.

5. That occasionally, though rarely, the summation of rapidly succeeding nervous impulses may be only incompletely effected within the nerve-cells of the spinal cord, or may not occur at all. In these cases results similar to those of Franck and Pitres are obtained.

A more detailed account of these experiments will shortly be published in the "Journal of Physiology."

December 17, 1885.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor Horace Lamb (elected 1884) was admitted into the Society.

The following Papers were read:—

I. "An Experimental Investigation into the Form of the Wave Surface of Quartz." By JAMES C. MCCONNEL, B.A. Communicated by R. T. GLAZEBROOK, M.A., F.R.S. Received November 9, 1885.

(Abstract.)

The paper contains an account of a number of measurements of the well-known "dark rings" of quartz. Each ring is due to one wave being retarded in the quartz behind the other by an integral number of wave-lengths, so the measurements give the directions through the plate of quartz corresponding to a series of known retardations. The relative retardation is, especially in a crystal of weak double-refracting power like quartz, mainly dependent on the distance between the two sheets of the wave surface. Thus my observations really give the separation between the two sheets at various points, and it is in this separation that the peculiarities of quartz are most strongly marked, and the various expressions put forward by theory most widely divergent.

With a plate cut at right angles to the axis, I obtained values of the separation from $\phi=4^\circ$ to $\phi=39^\circ$—ϕ being the angle between the ordinary wave normal and the axis—and with a plate cut parallel to the axis I obtained values from $\phi=53^\circ$ to $\phi=90^\circ$.

An obvious danger in this mode of investigating the wave surface

is the influence of the refraction effects. This is discussed at some length in the paper, and is shown to be in all probability negligible.

The measurements were taken with a spectrometer fitted up as a polariscope, whose great focal length and finely graduated circle were of much service.

I found it convenient to treat separately the region near the axis, where the abnormal form of the wave surface of quartz is most obvious. I have compared my results with nine different theories, each of which gives an expression of one of the two following forms.

$$D^2 = P_1^2 \sin^4\phi + D_0^2.$$
$$D^2 = P_2^2 \sin^4\phi + D_0^2 \cos^4\phi.$$

Here D is "the number of wave-lengths by which one wave lags behind the other in air, after the light has traversed normally a plate of quartz one millimetre thick, the normal to whose faces makes an angle ϕ with the optic axis." D_0 is the value of D when $\phi=0$, and is known from the rotatory power, and P_1 and P_2 are constants to which the theories assign different values. By inserting the observed values of D and ϕ, I obtained values of P_1 and P_2 from each ring. The results from one plate about 20 mm. thick were as follows :—

ϕ	4° 24′	5° 51½′	6° 51½′	7° 40½′	8° 23½′	9° 38′	11° 41′
P_1	15·054	15·207	15·220	15·260	15·249	15·258	15·269
P_2	15·220	15·293	15·290	15·311	15·295	15·292	15·292

Similar results were obtained from a second plate about 27 mm. thick.

From these figures I concluded that the second expression was the correct one, and that $P_2 = 15{\cdot}30 \pm {\cdot}01$. There is a considerable discrepancy in the case of the first ring, of which two possible explanations are given in the paper.

Cauchy gives $P_2 = \frac{a-b}{a^2\lambda} = 15{\cdot}351$, where a and b are the wave velocities perpendicular to the optic axis.

$$\text{Lommel gives } P_2 = \frac{1-a^2}{1-b^2}\,\frac{a+b}{2a}\,\frac{a-b}{a^2\lambda} = 15{\cdot}178.$$

$$\text{Kettler} \quad ,, \quad P_2 = \frac{a+b}{2b}\,\frac{a-b}{a^2\lambda} = 15{\cdot}486.$$

$$\text{Sarrau} \quad ,, \quad P_2 = \frac{a+b}{2a}\,\frac{a-b}{a^2\lambda} = 15{\cdot}306.$$

The other five, MacCullagh, Clebsch, Lang, Boussinesq, and Voigt, have the first form of expression and give $P_1 = \frac{a+b}{2a}\,\frac{a-b}{a^2-\lambda} = 15{\cdot}306.$

Thus Sarrau alone succeeds in explaining the observations satisfactorily.

For the larger values of ϕ, I calculated on Sarrau's theory what value of $a-b$ was required to give the retardation observed in each ring, and obtained as follows:—

ϕ	15° 14′	18° 21′	23° 16½′	28° 7½′	32° 7′	35° 34′	38° 36′
$a-b$ ...	·0037916	·0037922	·0037927	·0037931	·0037939	·0037932	·0037936

The observations on the plate cut parallel to the axis gave—

ϕ	53° 58′	57° 11′	64° 45′	72° 13′	79° 53′	83° 40′	85° 37′
$a-b$ ...	·0037949	·0037947	·0037945	·0037944	·0037943	·0037946	·0037944

In this last set the retardation amounted to about 300 wave-lengths, so that the bands corresponding to the two D lines were considerably separated, and the bands seen were consequently ill defined. Fortunately, however, a large angular error only produced a small change in $a-b$, so that these results are far more accurate than those of the other two sets. For $\phi > 50°$ the rotatory term is negligible, and Sarrau's theory is reduced to the simple Huyghenian construction. So our observations show that for $\phi > 50°$ the Huyghenian construction represents the wave surface in quartz with great accuracy.

From $\phi = 15°$ to $\phi = 50°$ there is a slight but persistent increase of $a-b$ with ϕ. It is shown in the paper that this may be satisfactorily accounted for by using the more general form of Sarrau's wave surface, viz., $(s^2-a^2)(s^2-a^2\cos^2\phi-b^2\sin^2\phi)$.

$$=\frac{4\pi^2a^2}{\lambda^2}(g_2\cos^2\phi+f_1\sin^2\phi)(g_2\cos^2\phi-g_1\sin^2\phi).$$

Here s is the wave velocity and a, b, g_2, f_1, g_1 are constants, g_2 depending on the rotatory power. Up to this point we have been supposing f_1 and g_1 are negligible. If now we make $8\pi^2a^2g_2(g_1-f_1) = \cdot 00033(a^2-b^2)^2\lambda^2$, we find that Sarrau's theory agrees with observation throughout within the limits of experimental error, even in the case of the first ring.

The observations were taken in the Cavendish Laboratory, Cambridge, during the months of March and June, 1885.

For full details as to the apparatus, the plates of quartz used, the mode of observation, the precautions necessary, the temperature effects, and the calculations, reference must be made to the paper.

II. "Second Report on the Evidence of Fossil Plants regarding the Age of the Tertiary Basalts of the North-East Atlantic." By J. STARKIE GARDNER. Communicated by Sir J. D. HOOKER, K.C.S.I., F.R.S. Received November 30, 1885.

I have the honour to make known the results attending the expenditure of £75, placed in my hands by the Government Grant Committee for the purpose of investigating the fossil floras of Scotland, which has been expended at Ardtun Head.

The position and physiography of this headland in the Isle of Mull has been fully described by the Duke of Argyll. It is the point of land separating Loch Laigh and Loch Scridain, and is about two miles in circumference and a mile across.

It is composed mainly of two sheets of basalt with remains of a third sheet, on some eminences and along the shore of Loch Laigh. These are almost horizontal, with a slight dip east, up Loch Scridain, and a considerable dip in the same direction up Loch Laigh. The upper sheet is not less than 40 to 50 feet thick, crystallised into rude massive columns, now much fissured and weathered, whilst the lowest presents a thickness of 60 feet, visible above low water, the upper two-thirds being amorphous, and the rest fashioned into slender and most perfect columns, bent in every direction, like those of the Clam-shell cave at Staffa. The beds are so exceedingly horizontal towards the seaward direction, that no one can doubt the columnar basalts of Staffa and the Treshnish Isles, Geometra and the mainland of Mull, being on the same horizon, if not parts of the same sheet. Between the two great lava beds at Ardtun is intercalated a bed of sedimentary deposit, reaching a maximum of 60 feet thick, and consisting of pale very fine-grained clay and limestone at the base, then sand and gravel, black laminated shales, whinstone, gravel, and laminated sands. The gravels are made up of flint, pebbles, and subangular rolled fragments of older lava beds, in a matrix of broken-down volcanic material. They present all the ordinary lines of current bedding, beautifully weathered out, and the pebbles are drifted precisely as in ordinary river gravels.

There can be no question whatever, indeed, but that the gravels are the deposits of the waterway of a river, and the shales, its overflows and backwaters. The river must have been of some magnitude, as its deposits traverse the whole seaward face of the headland, and their extension inland seems to be marked by the occurrence of two beds of coal, which have, to a small extent, been worked, one near Bunessan and the other nearer Ardtun village. An intrusive sheet of fine compact basalt rises on one side of the head, cutting a devious way

through each bed in turn, and dipping beneath the sea at the other extremity. On the coast, near the centre of the head, occurs a small chine, apparently due to the weathering out of a vertical dyke, which has cut through the gravels and shales, and left them in a very accessible position; it was here, accordingly, that I resolved to excavate them.

With the assistance of the Duke of Argyll's energetic factor, Mr. Sinclair, men and tools and a barrel of powder were forthcoming, and after a few days' work and the removal of a mass of the intensely indurated shingle bed, to the extent of perhaps hundreds of tons, many square yards of the whinstone and the underlying black shales were exposed. The large specimens of *Platanites aceroides* and *Onoclea hebridica*, now exhibited, were the results. The new specimens of the latter, including fertile as well as barren fronds, give a very different idea of its growth to what would have been imagined from those formerly known, and we see that the pinnæ were produced on very long and stout naked stems. The ravine, however, proved an unfortunate selection, for the whinstone became poorer in fossils as we got farther in, and the underlying black shales, though crowded with leaves, proved so squeezed and full of slickensides or faulted surfaces, and, consequently, so brittle as to be practically valueless for collecting purposes. The lighter limestone is wholly absent at this spot. From the condition of the shales and calcined appearance of the gravels—here of a steely-grey colour, intensely hard, with pure white and occasionally cherry-coloured flints, it is evident that the ravine must be the site of an old dyke, and if proof were wanting of a violent upthrust at this spot, it is to be found in the upturned edges of the bottom bed on the west face. The succession of beds in the section we had been so laboriously working at in the ravine, in no way prepared me for the discovery I made subsequently, that within 100 yards there existed, many feet below the lowest sedimentary bed present in the ravine, a deposit of limestone, rivalling in fineness and texture the celebrated lithographic stone of Solenhofen, and containing, as expressed by Professor Newberry, "most exquisitely preserved leaves." On removing some turf, in order to obtain a true section, I was overjoyed to find this deposit, so completely different in character to any previously seen in the basalts. It may seem strange that it should have been overlooked by the many geologists who have visited the spot; but the beds are in a very inaccessible position, and to work them dangerous, until sufficient had been worked away to afford standing room. Quarrymen could not be induced to work there at all, but two boatmen did not make any objection.

It is too early to attempt as yet to give any account of the new flora, which seems to differ considerably from that of the shales above. Very large leaves of many kinds occur in the clay at the base, which

was so friable, however, that only fragments could be secured. The leaves in the limestone are smaller and very sparsely scattered through it; there are, moreover, no cleavage planes, and hence much patience is required to obtain and develop them. I have obtained about twenty species of dicotyledon from it, the most prevalent being *Grewia crenata*, Hr., and *Corylus MacQuarrii*, Forbes, and *Acer arcticum*, Hr., all of which are also found in beds of the same age in Greenland. There are no ferns and only three conifers, a large variety of *Guilago adiantoides*, Unger, a new *Podocarpus*, the most northerly species I believe yet found, and *Taxus Campbelli*. The fragments from the clays show about eight additional species, and altogether I should judge that both floras were very rich. All the conifers occur also in the shales, and a specimen of *Guilago* has long been in the collection at Inverary. The most characteristic plants of the shales are those described by the Duke of Argyll and Edward Forbes, *Platanites aceroides* and *Rhamnites multinervis*. *Taxites Campbelli* is not, as affirmed by Heer, identical with *Sequoia Langsdorfii*, but appears to be a true *Taxus*. Some other leaves are certainly referable to *Protophyllum*, and we have representatives apparently of leaves determined as *Alnus*, *Cornus*, *Berchenia*, *Populus*, and *Corylus*—but among them there are none, so far as I can ascertain, that have ever been found in European beds of Miocene age. This is a point, however, upon which I do not wish to insist at present, farther than to say, that the flora seems to bear a *primâ facie* resemblance to cretaceous floras of America rather than to any yet known from Europe. The resemblance of the Coniferæ to those indigenous to China at the present day, is too remarkable to be overlooked.

It has become evident that the fluviatile rocks of the British basalts are of far greater extent and importance than had hitherto been imagined. Before the complete account of them, which I hope later on to prepare, can be proceeded with, their position in the series has to be fixed, their lateral extension to be mapped.

The first of these points is fortunately not difficult to settle. The base of the basalts is exposed at Burg Head on the opposite side of Loch Scridain, resting upon Jurassic rocks and fragmentary masses of chalk. The base of the series seems formed of two immense sheets of ash, the lowest of which is full of scoriæ, and about 100 feet above these, resting upon columnar basalt, in every respect similar to that of Ardtun Head, are fluviatile beds, sands and clays from 9 to 12 feet thick. Overlying these is a bed of rudely columnar basalt, and taking account of all the circumstances, there cannot be much doubt about the fluviatile series on both sides of the loch being upon the same horizon. The beds are, in fact, seen to be horizontal to the west as far as the eye can reach. The horizon of the Ardtun gravels would, therefore, seem to be about 150 feet from the base of the series.

Taking into account the superior thickness of the basalts in Mull, and above all the presence of ash beds at their base, it seems probable that they were nearer the vents than Antrim, and that their lowest beds are at least not newer, so that the Mull leaf-beds at 150 feet from the base should be much older than the Glenarm and Ballypalady leaf-beds at 600 feet from the base.

The horizontal extent of the fluviatile beds of Mull is more difficult to estimate. Gravel is mentioned as present at Loch Truadh, to the north-west, and at Carsaig to the east. In the latter locality it is, perhaps, thicker and more extensive than at Ardtun. The horizon should also be found in two of the Treshnish isles and round the north-west coast of Mull, and there can be little doubt but that deposits of plants exist in many localities besides Ardtun. Black shales, with identical leaves, have been found in Canna, and leaflets of *Taxus* or a similar foliaged conifer at Uig.

Though the fluviatile beds at Bourg are unfossiliferous, a very interesting relic of the Eocene vegetation occurs there, for a large tree, with a trunk 5 feet in diameter, has been enveloped as it stood to a height of 40 feet, by one of the underlying lava beds. Its solidity and girth enabled it to resist the fire, but it subsequently decayed, leaving a hollow cylinder filled in with *debris* and lined apparently with the charred wood. There is also the limb of a larger tree in a fissure not far off. The wood proves to be coniferous, belonging possibly to the *Podocarpus* whose leaves are so conspicuous in the beds above.

III. "Addition to a former Paper on *Trichophyton tonsurans.*" ("Proc. Roy. Soc.," vol. 33, p. 234.) By GEORGE THIN, M.D. Communicated by Prof. M. FOSTER, Sec. R.S. Received December 3, 1885.

In the "Proc. Roy. Soc.," vol. 33, p. 234, 1881, a paper of mine is published on "*Trichophyton tonsurans.*"

Since the time that the investigations recorded in that paper were made, the study of parasitic fungi has been greatly facilitated by the introduction by Dr. Koch of gelatinised meat juice as a medium for cultivation, and having again studied the development of trichophyton with the advantages derived from the use of this medium, I believe that two results which I have obtained might be usefully published as a supplement to the paper referred to.

The gelatinised meat juice which I used was peptonised and neutralised, and trichophyton grew on it readily and with certainty.

In my previous experiments (in which I used vitreous humour) I had succeeded in growing the fungus when the hairs containing the

spores were floated on the surface of the fluid, but I had not succeeded in finding evidence of growth when the hairs were submerged. This result must have been due to some accidental cause which escaped undetected, as I have found that trichophyton grows when the hairs are entirely submerged in the gelatinised beef juice, and the spores are excluded from contact with the air.

In my previous paper I described a number of experiments which appeared to show conclusively that trichophyton is not related to the ordinary fungi (*Penicillium*, *Mucor*, &c.) with which it had been up to that time confounded. The experiments with gelatinised meat juice confirm that view. In many instances in which I cultivated trichophyton on this medium I never once observed organs of fructification or appearances that suggested that it could be identified with common fungi. As regards *Penicillium glaucum* with which, from the abundance of its spores in the atmosphere of laboratories, cultivations are most likely to be, and are most often fouled, the difference can be shown by a very simple experiment.

A layer of the gelatinised meat juice is poured over a pure slide and allowed to cool under proper precautions. Ringworm hairs and penicillium spores are "sown" in parallel lines on the surface of the medium, and the slide is put in a moist chamber at ordinary temperatures. If the slide is examined in from twenty-four to forty-eight hours the penicillium will be found to be growing with great rapidity, whilst the mycelium of trichophyton has made in comparison very little growth. Repeated crops of penicillium may be grown up to the stage in which the characteristic organs of fructification are fully developed, whilst during the same time the trichophyton mycelium grows steadily and slowly, with no distinct signs of spore formation and no trace of organs of fructification.

I may add that, in a recent letter to me, Dr. Koch states his conviction that trichophyton is a distinct and independent kind of fungus.

IV. "A New Form of Spectroscope." By J. NORMAN LOCKYER, F.R.S. Received December 5, 1885.

Some two or three years ago, when the sun-spot work carried on at Kensington revealed the different behaviour in different spots of lines visible in the spectra of the same element, it seemed desirable to extend similar observations to metallic prominences, and, if possible, in such a way that comparisons over a considerable reach of spectrum should be possible.

It then struck me that a grating cut in half, with one part movable, would afford a ready means of doing this.

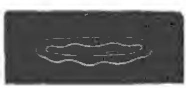
Circumstances prevented the realisation of this scheme till quite recently, when I put into Mr. Hilger's hands a grating presented to me by Mr. Rutherfurd.

The result is excellent. It is possible to observe C and F, for instance, together quite conveniently, with either a normal or a tangential slit. The only precautions necessary are to see that half of the light passing through the object-glass falls on the half grating, and that the rays which come to a focus on the slit plate are those the wave-lengths of which are half way between the wave-lengths of the two lines compared.

V. "On the Formation of Vortex Rings by Drops falling into Liquids, and some allied Phenomena." By J. J. THOMSON, M.A., F.R.S., Fellow of Trinity College, Cavendish Professor of Experimental Physics, Cambridge, and H. F. NEWALL, M.A., Trinity College, Cambridge. Received November 28, 1885.

When a drop of ink falls into water from not too great a height, it descends through the water as a ring, in which there is evidently considerable rotation about the circular axis passing through the centres of its cross sections; as the ring travels down through the water inequalities make their appearance: more ink seems to collect in some parts of it than in others, and as these parts of the ring descend more rapidly than the rest, it assumes some such appearance as that shown in fig. 1. These aggregations as they descend develope

FIG. 1.



fresh rings in the same way as the original ring was developed from the drop. The ring is thus split up into several rings, each of which is connected with its neighbours by threads of ink affording a very pretty illustration of the continuity of motion in a liquid (see fig. 2).

As the secondary rings descend they develope other rings just as they were developed from the original one, and the process of reproduction seems to go on indefinitely.

It is not every liquid, however, which, when dropped into water, gives rise to rings, for if we drop into water any liquid which does not mix with it, such as chloroform, the drop in consequence of the

FIG. 2.

surface tension remains spherical as it descends. In fact, we may say that, with some few exceptions to be noticed later, rings are formed only when a liquid is dropped into one with which it can mix. This is important, because surface tension has been supposed to play an important part in the formation of these rings; it is difficult, however, to see how any appreciable surface tension can exist between liquids that can mix, and as far as our experiments go they tend to show that it is only the absence of surface tension which is necessary for their production. There are, as we shall show later, many cases where rings are formed under circumstances in which there is no possibility of capillary action, such as when the liquid into which the drop falls is the same as the drop itself. As capillarity was found not to be involved in the production of the rings, it seemed interesting to investigate the subject further, and the following investigation was undertaken with this object. As the result of our experiments we have been led to a theory of the production of these rings of which we shall now give a brief sketch in order to render the sequence of our experiments more intelligible.

Let us suppose that a spherical drop falls into a liquid; the motion of the liquid surrounding the drop will at first be much the same as if a solid sphere of the same size were to fall into the liquid. Now, when a sphere moves through a liquid the tangential velocity of the liquid is different from the tangential velocity of the sphere, so that the liquid flows past the sphere. If the sphere be fluid as well as the medium in which it moves, there will not be an absolute discontinuity in the motion, but only a very rapid change, so that there is a finite alteration in an exceedingly small distance. This alteration is equivalent to a vortex film covering the sphere, the lines of vortex motion being horizontal circles, and if the liquid be viscous the vorticity in the film will diffuse inwards and outwards. As the drop falls the resistance makes it get flatter and flatter until it becomes disk-shaped; by this time, however, it is full of vortex motion, and as the disk-shape is an unstable arrangement of vorticity, the disk must break up into the stable arrangement—that of an anchor ring. This is a

rough outline of the theory we adopt. It will be seen that the most important property of the liquid concerned is its viscosity—the viscosity must be such that when the drop has become disk-shaped there should be enough vortex motion in it to cause it to break up; if the viscosity is too small the vortex motion in it will not have had time to spread far by the time the drop has become disk-shaped, and so the drop will continue to flatten and get into thin sheets with streaks of vortex motion in it instead of breaking up into a ring, whilst if the viscosity is too great the vortex motion will be dissipated before the drop becomes disk-shaped.

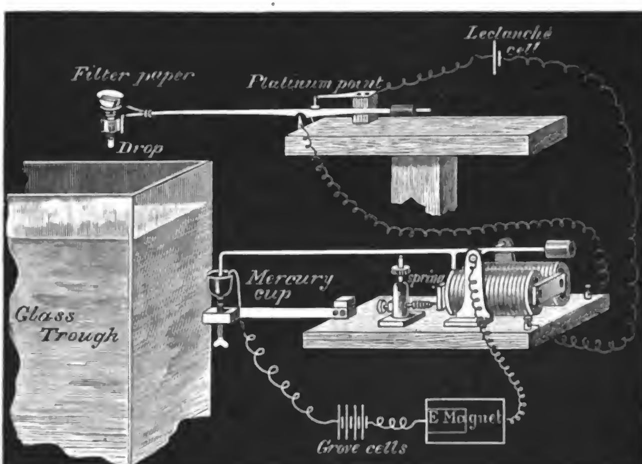
We shall now give the experiments which led us to form the conclusions. We began by investigating the change in the shape of the drop before it became disk-shaped.

Shape of the Drop before it becomes a Ring.

To study the change of shape of the drop as it falls through the liquid we have had recourse to instantaneous illumination, and have used for this the bright spark formed at breaking in a mercury cup a circuit containing an electromagnet. It was necessary that we should be able to illuminate the drop at any point of its fall, and it was obviously convenient to make the actual fall of each drop start a set of processes which should result in the spark. The figure shows diagrammatically the arrangement we have used.

The drop is formed at the end of a small piece of glass tubing

FIG. 3.



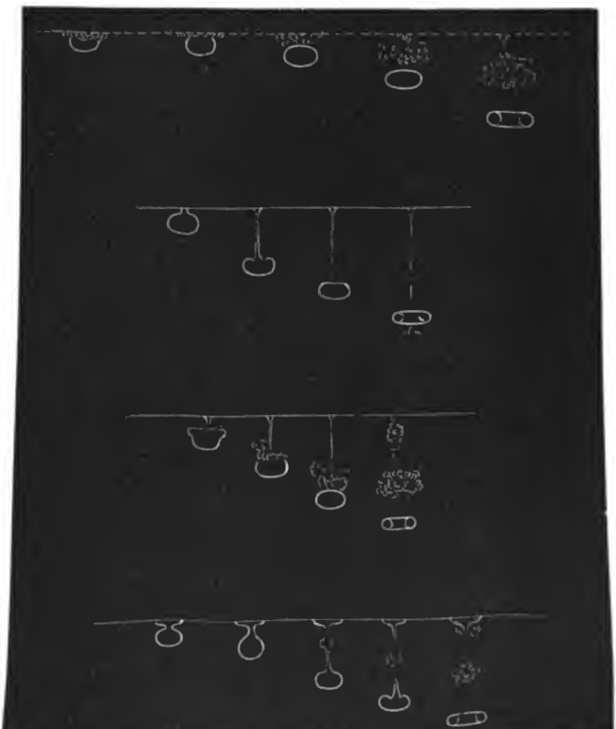
$\frac{3}{16}$ inch in diameter, the liquid filtering slowly through a small filter paper held by a wire ring in the upper end of the glass tube. The tube and filter are fixed at one end of a light lever, which is counterpoised at the other end and balanced carefully on a knife edge. The lever was made of a narrow strip of brass foil, folded for the sake of increased rigidity into a Λ-shape. On the lever between the knife edge and the adjustable counterpoise a small piece of platinum is fixed, and when the balance is set this is pressed lightly upwards against a platinum point which is held in position by a brass arm. The knife edge is connected with one terminal, the brass arm with the other terminal of a circuit containing one or two Leclanché cells and a Morse sounder, which we have adapted so as to serve as a contact breaker in the spark circuit. The moving brass lever of the Morse is prolonged by means of a light wooden arm, and the arm carries a wire tipped with platinum and bent so as to dip into a mercury cup. A current from three or four Grove cells is passed along this wire, and the circuit is completed through a strong electro-magnet. The mercury cup is placed conveniently so as to illuminate the drop as it falls.

In using the apparatus the following is the procedure:—Several drops are passed into the filter paper, and the counterpoise is adjusted so that contact is just made at the platinum points. Thus, the relay circuit is complete, and as a consequence the lever of the Morse is held down, and so the electromagnet circuit is made by the dipping of the platinum tip into the mercury cup. The spring and counterpoise of the relay and the mercury cup have been "set" so as to give the spark at break at about the moment required. When the drop detaches itself from the tube the equilibrium of the balance is upset, the counterpoise sinks, and so breaks the relay circuit, which consequently breaks the electromagnet circuit at the mercury surface, and the spark so produced illuminates the drop at some point in its descent. This point may be varied by altering the "setting" of the relay and mercury cup.

Various liquids were used for dropping: fluorescein dissolved in weak ammonia gave very good results for two or three drops when dropped into water, but it soon diffused through the column of water, and the drops were then no longer clear, as the efficient rays had been absorbed at the surface of the column. Milk gave excellent results; but even when considerably diluted it is disagreeable to work with on account of the greasiness that accumulates in the dropping tube, and of the cloudiness produced in the column. A weak solution of nitrate of silver dropped into a column of weak sodic chloride solution was found to do best, as very good rings are formed, and the precipitate may be discharged by the addition of a few drops of ammonia. The results figured below are those obtained in this way.

We have observed a great number of drops at many different points of their fall through the column into which they have been dropped, and at any given instant have found that a few definite forms repeat themselves. Thus we are led to believe that the conditions of fall, though seemingly not altered, vary in a few definite ways, so that successive drops do not always pass through the same series of phases. Indeed, simple observation in continuous light shows clearly differences in the perfection of formation of the rings under apparently similar conditions. We have found a set of forms for a particular instant, and from the various sets taken for successive instants, separated by small intervals, we have picked out series of forms which strike us as continuously derivable from one another.

Fig. 4.



It will be seen at a glance from the figures that the variations are only in the unimportant parts, whilst the essential parts are recognisably the same. At the early stages the drop is more or less spherical; but as it descends, it gets flatter and flatter, as we might expect since its motion is resisted, and at one of the stages becomes almost disk-shaped, and passes very quickly from this into the ring shape.

This change of shape, we imagine, is due to the presence of the vortex motion, the distribution of rotation in the ring being a stable arrangement of vortex motion, whilst that in the disk is not. The vortex motion in the drop has travelled from the boundary, diffusing according to the same laws as those which govern the conduction of heat, the quantity corresponding to the diffusivity in the conduction of heat being in this case μ/ρ, where μ is the coefficient of viscosity and ρ the density of the liquid. If μ/ρ be very small, the vortex motion will not travel far into the drop, whilst if it be large it will all have diffused before the ring has become disc-shaped. In neither of these cases should we expect the disk to change into a ring, and it will be seen later on that as a matter of fact it does not.

Effect of Dropping one Liquid into another.

A drop does not always make a ring when it falls into a vessel containing liquid. We have tried the effect of letting a drop of one liquid fall into a vessel containing another liquid for a good many substances. The results are given in the following table :—

Column of Water :—

- Rings are formed when drops of the following liquids are let fall into a column of water : milk, alcohol, blood, aqueous solutions of sugar, of gum arabic, of potash, of permanganate of potash, of carbonate of potash, of sodic chloride, of copper sulphate, of nitrate of potash, of oleate of soda, of nitrate of silver, of cobalt chloride, of carbonate of soda, of ammonic chloride, hydrochloric acid, acetic acid, nitric acid, Plateau's soap solution, essence of carraway, solution of iodine in ammonia, solution of fluorescein in ammonia, glycerine and water, sulphuric acid.
- Globules are formed when drops of the following liquids are let fall into a column of water: carbon bisulphide, chloroform, ether, olive oil, paraffin oil, turpentine.

Column of Paraffin Oil :—

- Rings: carbon bisulphide, chloroform, ether, olive oil, turpentine, butylic alcohol.
- Globules: water, alcohol, essence of carraway, glycerine and water, dilute sulphuric acid.

Column of Alcohol:—

Rings: carbon bisulphide, chloroform, water, turpentine, butylic alcohol, sulphuric acid.

Globules: olive oil, paraffin oil.

It will be seen from the tables that a drop of one liquid only makes a ring when let fall into another liquid, when the two liquids can mix, and, therefore, when the surface tension is very small.

The following experiment shows that an exceedingly small amount of surface tension is sufficient to prevent the formation of the rings. Absolute alcohol dropped into benzene gives rings; water gives globules: to about 10 c.c. of absolute alcohol water was added drop by drop, the mixture was stirred, and a drop was let fall into benzene. Until after the third drop of water had been added, little change in the appearance was noticed; a ring was formed, and this subdivided into secondary rings, and so on. After the fourth drop was added, very small globules began to appear after a good many subdivisions. After the fifth drop was added, the ring first formed subdivided not into rings but into flattened globules. After the sixth or seventh drop was added, the appearance of the primary ring changed; there seemed to be a more definite surface to it; in fact a small surface tension had sprung up. The globules formed on subdivision of the ring were quite disconnected from one another, whereas before there had been trails or festoons following each. After the seventh or eighth drop of water was added, the formation of the primary ring seemed uncertain; the flattened globule, if large, broke up irregularly into smaller globules without the intervention of the ring shape. This experiment shows that if a liquid A forms spheres when let fall into another B, then A may be diluted with more than 1000 times its volume of some liquid, which has no surface tension with B, before it loses the property of making spheres.

The most striking proof, however, that the formation of the rings does not depend on surface tension, is the fact that the rings are formed when the liquid of which the drop is made is the same in all respects as that into which it falls. If we take a vessel full of water, and raise from it by means of glass tubing enough water to make a drop, then, when this drop falls back again into the vessel from the proper height, it forms a ring. After a little practice, it is easy to distinguish the ring from the rest of the liquid, and this may be done still more easily if we mix some insoluble powder with the water.

Experiments with one Liquid.

We found on trying different liquids that they behaved very differently when treated in the way we have just described. In some of them a distinct ring was formed by the drop, whilst in others the

drop after falling into the liquid seemed to spread through it without assuming any definite shape. We found when we used various liquids that if the drops were let fall under similar circumstances, the results depended only on the value of μ/ρ (the kinematic coefficient of viscosity for the liquid). This is shown in the following table, where the behaviour of the drop and the value of μ/ρ are given for several liquids, which we have divided into classes. The viscosity coefficient μ has been determined by Poiseuille's transpiration method; that is, from the time in which a constant quantity of liquid flows under constant pressure through a fine capillary tube. To test the character of the rings formed in the case of any particular liquid, drops of it are let fall into a column of it from three different heights: 1st, such that the drop just touches the surface of the liquid column at the moment it detaches itself from the tube; this height of fall is denoted in the table by "fall = 0"; 2nd, such that the tube is held $\frac{1}{2}$ inch above the surface of the column; 3rd, such that the tube is held $\frac{3}{4}$ inch above the surface. In describing the character of the rings we have used terms which it will be well to define. "*Splash*" denotes the irregular spread of the drop through the liquid column; it takes place with whirls and eddies irregularly, and is difficult to represent, but the figure (5) would probably recall the appearance to

Fig. 5.

one who had seen the reality. "*Uncertain*" expresses that a ring or a splash is formed, one as often as the other. "*Blob*" denotes the appearance of a drop that does not break and spread through the column, but remains within its boundary; the figure (6) represents

Fig. 6.

the case. "*Doubtful*" denotes that the drop tends to become a ring, but that it is a question whether it ever leaves the state of a blob.

Class	Liquid	ρ. Spec. gravity at 17° C.	μ. Transpiration time. Water = 1.	μ/ρ. Kin. visc. coefficient.	Character of ring formed. Fall = 0.	¼ inch.	¾ inch.
Class I, or Ether Class.	Carbon bisulphide	1·271	0·467	0·418	Uncertain.	Splash.	Splash.
	Chloroform.............	1·484	0·6951	0·589	Uncertain.	Uncertain.	Splash.
	Ether..................	0·7458	0·4428	0·654	Fair ring.	Uncertain.	Splash.
	Benzene	0·705	0·494	0·700	Fair ring.	Generally ring.	Splash.
Class II, or Water Class	Water..................	1·0	1·0	1·0	Good ring.	Good ring.	Very good ring.
	Oleate of soda (i)........	1·003	1·226	1·222	Slow ring.	Fair ring.	Fair ring.
	Absolute alcohol	0·7963	1·206	1·514	Slow ring.	Good ring.	Fair ring.
	Turpentine......... ...	0·879	1·753	1·99	Doubtful ring.	Fair ring.	Good ring.
	Sulphuric acid III (ii) ...	1·419	3·804	2·680	Fair ring.	Fair ring.	Good ring.
	Paraffin oil I (iii)........	0·803	2·176	2·71	Doubtful ring.	Fair ring.	Fair ring.
	Glycerine II (iv)........	1·085	2·951	2·72	Blob.	Slow ring.	Fair ring.
Class III, or Butyl Class.	Sugar (v)..............	1·157	4·512	3·90	Blob.	Blob.	Slow ring.
	Paraffin oil II (vi)	0·820	3·326	4·050	Very slow ring.	Very slow ring, quickly stopped.	Slow ring, quickly stopped.
	Butylic alcohol	0·8051	3·469	4·31	Very slow ring.	Fair ring.	Fair ring.
	Sulphuric acid II (vii) ..	1·589	8·217	5·053	Very slow ring.	Doubtful ring, not quite breaking.	Fair ring.
Class IV.	Sulphuric acid I (viii)....	1·827	27·02	14·79	Blob.	Blob.	Blob.
	Glycerine I (ix).........	1·179	21·55	18·28	Blob.	Blob.	Blob.

Notes.—(i.) Oleate of soda solution in water, made of that strength which best gave certain results when a drop was let fall into a special sample of paraffin oil (see below).

(ii.) Sulphuric acid III, a mixture of acid and water, made by adding water to strong acid until drops taken from the mixture and let fall into it gave rings as good as many of the liquids in Class II.

(iii.) Paraffin oil such as is ordinarily used in lamps.

(iv.) Glycerine II, a mixture of glycerine and water, dilute enough to bring it only just into Class II.

(v.) Sugar solution in water, so strong as just to give rings when dropped into itself.

(vi.) Paraffin oil II, a special sample of oil that we have not been able to match, and which will be referred to below (p. 432).

(vii.) Sulphuric acid II, made of such strength that it should belong by the character of its ring to Class III.

(viii.) Sulphuric acid I, strong commercial acid.

(ix.) Glycerine I, diluted with water, but still of such strength that rings were never formed by drops of it falling into a column of it.

These experiments indicate that the nature of the motion, after the drop has fallen into the liquid, depends upon the value of μ/ρ. Now μ/ρ is a quantity of the dimensions of the product of a length and a velocity, so that nothing can depend upon the absolute value of this quantity, but only on the ratio of it to the product of some length and velocity in the system. The most obvious length in the system with which μ/ρ can be connected is the size of the drop or ring. If this is so, diminishing the size of the drop when μ/ρ is kept constant ought to produce the same effect as increasing μ/ρ when the size of the drop is kept constant. Now if we take a liquid from Class I, the effect of increasing μ/ρ sufficiently would be to make it behave like one in Class II, that is, give very much better rings, so that for a liquid of this kind we should expect that small drops would make better rings than large ones. To test this, we repeated the experiments with the liquids of Class I, using much smaller drops than we used before, and we found that now they made rings much more readily and certainly, in fact, with this sized drop, they deserved to be put into Class II. If we try the same thing with liquids in Class III, we find that the rings are worse with small drops than with large. On the other hand, increasing the size of the drop when μ/ρ is kept constant, should have the same effect as diminishing μ/ρ when the size of the drop remains constant, so that if we repeat the experiments with liquids from Class IV, using larger drops than before, we should expect to get better rings. Experiment showed this to be the case. Large drops were obtained by dipping a piece of glass tubing into the liquid (*e.g.*, strong sulphuric acid), and then raising the tube after closing the top with the finger; on removing the finger, a considerable quantity of liquid flows through the tube, making a large ring. The velocity with which μ/ρ would be most

naturally connected is the velocity of the drop; it is not possible, however, to alter this very much without introducing great disturbance into the liquid.

These results admit of very easy explanation, if our theory of the formation of the rings is correct. According to this, the reason that the liquids in Class I do not make rings, is because the vortex motion has not penetrated sufficiently into the drop to make it break up, when it becomes disk-shaped. If, however, the drops be made smaller, the vortex motion has a better chance of filling it before it becomes disk-shaped, and so causing it to break up into a ring. When μ/ρ is very large, as is the case for liquids of Class IV, the vortex motion is dissipated so quickly, that though the drop may have been filled with vortex motion, this has all diffused away before the drop reaches the disk-stage. If we make the drop larger, it will take longer to get full of vortex motion, and there may be enough left in, by the time it gets disk-shaped, to cause it to break up into a ring. There is another way in which the formation of rings by liquids of Class IV may be promoted: suppose that, instead of letting a drop fall into a column of the same liquid, we let it fall into another liquid for which μ/ρ is smaller; then since this liquid is a worse conductor of vortex motion than the drop, the vortex motion will not diffuse so readily into the surrounding liquid. Thus we should expect the drop to form a ring more readily than before; and we have found this to be the case. Thus, for example, drops of sulphuric acid I from Class IV, let fall into either sulphuric acid II of Class III or sulphuric acid III of Class II, give rings. Similarly with solutions of sugar, of caustic potash, and of glycerine.

These effects are sometimes masked by the effects produced by difference of density; for if the drop is much heavier than the liquid into which it falls, it will fall faster, and this will promote the formation of the ring. If we guard against this source of error, we may see that if a drop does not make rings when it falls into a column of the same liquid, it will not make rings when it falls into a column of another liquid of the same density, but for which μ/ρ is greater; but it may make rings if μ/ρ be less than for the drop.

On the Splitting up of the Rings.

When a ring has travelled some little distance through the liquid, its outline generally becomes irregular, and after a time takes the corrugated appearance shown in fig. 1. The corrugations become more and more marked as the ring falls, until the appearance is that represented in fig. 8, γ: the drops at the bottom of the bends develop rings in the same way as the ring itself was originally developed. This process of subdivision is repeated several times, until the ring assumes the appearance shown in fig. 7.

Fig. 7.

The separate rings always remain connected with each other by threads of liquid; this shows that there is no surface tension; for if there were, these threads would break up into drops. The only stage at which the motion is discontinuous is when the drops are changed into rings. The change from the ring shape to stage 2 (fig. 1) occurs quite gradually, and there is nothing analogous to the separation which takes place when a cylinder of liquid splits up into drops. We have made a great many experiments on the subject, and have arrived at the conclusion that two conditions are necessary for the splitting up of the rings.

1. The liquid forming the ring must be of density different from that of the liquid into which it falls.

2. There must be motion in the liquid into which the drop falls.

We suppose that the splitting up takes place in the following way. In consequence of the motion in the column of liquid the ring gets a little uneven, more of the liquid collecting in one part of the ring than another. Now, if there is plenty of vortex motion in the ring, this irregularity will not be permanent, as the anchor ring with uniform cross section is the stable form for the motion, so that unless the disturbing force is too great, the ring will oscillate about the anchor-ring shape, and the irregularity will not increase. If, however, the vortex motion in the ring be small, it may not be able to balance the disturbing forces and the irregularities will increase. The disturbance is due to the resistance of the liquid in the column, the places where the liquid in the ring has collected falling faster than the remaining portions of the ring; the ring consequently takes in time some such appearance as that shown in fig. 8 (γ and δ). The thicker portions will behave now as the drop did when it fell into the column, and they will develop rings in just the same way.

Fig. 8.

The observations on which we base these conclusions are the following:—

We have never seen a ring break up when the liquid forming the ring was the same in all respects as that surrounding it. If the temperature of the drop be different from that of the column, or if so much powder has been added to the drop as to make the difference of the density appreciable, there is breaking up.

The number and size of rings into which the original ring breaks, and their manner of distribution round its circumference, are quite irregular.

When the liquid into which the drop falls has been allowed to rest for some hours, a ring will go much further and will last much longer without splitting up, than when it follows on a succession of rings. Some rings of very dilute permanganate have thus been observed to last for as long as ten minutes.

When there is little difference in the density of the drop and the liquid into which it falls, the ring does not break up until there is no vortex motion in it.

When, however, the difference in density is large, the ring may break up while it is still rotating.

If a tube be drawn out into a fine capillary and be filled with sulphuric acid, and held so that its capillary end is just beneath the surface of a column of water, a fine stream of acid flows down; and on it marked beadings appear. Each bead gives rise to a vortex ring, and the rings so formed behave in characteristic manner (fig. 9.)

FIG. 9.

Here there seems strong evidence of a tension between the acid and the water, but the appearances are to be explained by differences of velocity in the stream, brought about by motion in the column of

water, or by vibrations communicated to the capillary tube. If the experiment be made with all care to avoid vibration, the stream falls unbroken through a column of 8 inches of water: whilst if a tap be given to the acid tube a break occurs in the stream, in consequence of a momentary stop in the flow of acid, a small bead is formed, and from it a ring. If no care is taken to avoid vibration the beads will follow one another very rapidly. It may be objected that if there existed a surface tension, it would only be when disturbances were communicated that beadings would appear. But in such a case, the resolution into drops would be complete, and small spherules would be formed between the larger drops. In fact, however, the connexions between the beadings are fine filaments of acid, so that the beadings are never really separated from one another. We have, moreover, convinced ourselves of the correctness of this explanation, by allowing a stream of cold water with lycopodium powder to flow from a fine tube into a column of slightly warm water; similar cessations in flow and formations of beadings may be observed; the rings are not well formed, but this is to be expected, for the conditions are not nearly so favourable.

Experiments with two Liquids between which there is Tension.

When there is a very small tension between the liquid of the drop and that in the column, some very interesting results are obtained. In a few cases a ring is formed for a moment, but is almost immediately broken up into drops and spherules. As instances of such cases we may mention strong caustic potash solution dropped into paraffin oil: strong sulphuric dropped into turpentine or into paraffin oil: a certain mixture of turpentine and alcohol dropped into paraffin oil: a mixture of alcohol and chloroform dropped into water: and the mixture of alcohol and a few drops of water dropped into benzene (see above, p. 423). In most cases the drop falls through the liquid shaped like a disk, and in the first part of its course changes its shape very considerably. These changes may be well observed in the case of a drop of sodium sulphate solution falling through paraffin oil; they are shown in fig. 10.

At first the drop is nearly spherical (α); then it becomes flattened (β); next it becomes quite flat underneath or sometimes even hollow (γ), the top remaining curved. During all this time the velocity of the drop has been changing. The top now gets flatter and flatter (δ, ϵ, ζ), while it begins to bulge out at the bottom till the drop is saucer-shaped (η, θ), hollow at the top, and rounded at the bottom, and we perceive that it is followed close on by a vortex ring in the liquid of the column. It now moves with constant velocity through the liquid as though it met with no resistance, differing in this respect from the case where the drop falls as a ring, when its velocity—unless th

FIG. 10.

difference of density is very great—continually diminishes. We see the reason for this to be that the vortex at the back of the disk since it tends to make the liquid move along its stream lines, will increase the pressure in the rear and thus diminish the resistance. In fact we see that what the vortex does is to make the conditions very much the same as if the saucer-shaped drop were moving in a stream flowing with the same velocity. The existence of the following vortex is shown in a very beautiful manner in the next stages represented at ι and κ in the figure: the edges of the saucer-shaped disk become thinned out to such an extent by the action of the radial streams above and below, that they are drawn out into fine filaments which immediately break up into small spherules, and these are carried round in the vortex behind the drop.

These latter stages are best shown in the case of a certain solution of oleate of soda let fall into a special sample of paraffin referred to above (p. 426), and the changes are represented in fig. 11.

If only a small drop of oleate is let fall, the steady state is reached before the spherules are detached, and the drop falls in the shape shown at *a*. If a slightly larger drop falls, then the form *b* is reached,

FIG. 11.

and persists until so much of the drop has been whirled off into the following vortex that the form *a* is reached again, and the velocity of fall never attains such a value as to bring about the thinning out of the edges. If a still larger drop is let fall, the forms C_1, C_2, and C_3 are assumed, in which the thin cylindrical part at the edge is at first vibrating up and down, until the steady phase, C_3, is reached: this will by degrees be reduced to the form *b*, but the column of paraffin was not long enough for us to observe the final form *a*. If a still larger drop of oleate is used, the vibrations of the cylindrical part, *d*, are so great that the drop is torn asunder and such disturbance is produced in the column that a steady fall is not attained. These results may be obtained by increasing the strength of the oleate solution and keeping the size of the drop constant.

One more case of interest may be described, namely, that of drops

of carbonate of soda dropped into paraffin oil. The phases are represented in fig. 12. The earlier ones are somewhat similar to those above described. But the phase shown at e is different. Here it seems quite a chance, as it were, that the disk does not break into a ring. The instability is immediately shown by the oscillations, which begin at this point and continue in a regular manner through a fall of more than 3 feet. An attempt is made to show this in the phases ζ, η, θ. A thickening appears at one side, and the opposite edge thins off considerably, and these irregularities travel regularly round the drop as it falls through the column.

Effect of the Height of Fall on the Formation of the Ring.

We have found that alteration of the height from which a drop falls before reaching the surface of the column modifies the formation of

FIG. 12.

the rings considerably. Good rings are formed only within small limits of height—from about 1 to 3 inches, according to the size of the drop. Above 3 inches it is only when the drop falls back into the column after rebound that a ring is formed, and that very irregularly. But within the limits for any particular size of drop rings are much

better formed from some heights than from others. We have observed many cases, varying the size of the drop, the liquid of which it is composed, and the tension of the surface, and have found that the variations in goodness of the rings—where goodness is judged by the length of path in the liquid—depend on the variations in the shape of the drop at the moment it touches the surface. Curves were plotted out, abscissæ being taken equal to the height of fall, and ordinates equal to the depth to which the ring goes without breaking up. The curves show two or more maximum points: the abscissa values for these differ from one another by lengths which may be reduced to time intervals. We find that there is very fair agreement between the intervals so calculated and the periods of vibration of the drop about the spherical form. With small drops of water there are several maxima close together, but more widely separated as the fall gets longer (fig. 13). The equivalent intervals are about $\frac{1}{85}$ of a second,

Fig. 13.

whilst the period of vibration of a drop of the size used is about $\frac{1}{90}$ of a second. With as large drops as can be formed from a tu' 13. there are only two maxima separated by an interval equivalent .c Club.

of a second. The period of vibration of such a drop was calculated to be $\frac{1}{21}$ of a second.

The effect of diminishing the tension would be to increase the period of vibration. In experiment we have observed that the separation of the maxima is much greater in the cases of paraffin and of turpentine and of alcohol. But difficulties were experienced in the fact that drops of these liquids when let fall from small heights up to about half an inch, generally assumed the spheroidal state. Equal drops of water and of weak oleate of soda solution were let fall into water, and it was found that the separation of maxima was nearly twice as great for the oleate as for the pure water.

In the case of large drops, drops let fall from a point midway between the points to which maxima correspond are nearly always broken up without the formation of a ring. In the case of small drops rings are nearly always formed, but the depth of their paths in the liquid varies from about half an inch to 5 inches.

VI. "A Preliminary Account of a Research into the Nature of the Venom of the Indian Cobra (*Naja tripudians*). By R. NORRIS WOLFENDEN, M.D. Cantab. (from the Physiological Laboratory, University College, London). Communicated by E. A. SCHÄFER, F.R.S. Received November 17, 1885.

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"The Influence of Bodily Labour upon the Discharge of Nitrogen." By W. NORTH, B.A., F.C.S. Communicated by J. BURDON SANDERSON, F.R.S. Received October 29, 1883. Read November 15.

[PLATES 5—10.]

As in the experimental researches previously published on the subject of the present paper, the methods employed have been for the most part entirely different from those which I have found best adapted to the purpose, I have after much consideration judged it inexpedient to lengthen an already long communication by giving an account of them.

The scope of my inquiry has been strictly limited to one question, viz., that of the influence of labour in modifying the normal relation between food and excreta. I have made no attempt to investigate the mode in which nitrogenous products came into existence in the organism.

The only published experiments of any importance which bear strictly on the subject of inquiry are those of Dr. Parkes,* contributed to the Royal Society in 1867 and 1871, and those of Dr. Austin Flint, on the pedestrian Weston, 1871.† The results of these two investigations were as follows:—

Dr. Parkes found that during or immediately after severe labour, the discharge of nitrogen was more or less increased; the increase, however, was so inconsiderable that it might well be questioned whether it could not be accounted for as dependent on the more perfect absorption of food, for although the diet of the soldiers experimented upon was carefully regulated, and the nitrogen it contained determined by analysis, with the result that before work the quantity of nitrogen taken in considerably exceeded the quantity discharged, the two became practically equal during the work period, consequently if the whole period of observation is taken into account, the nitrogen discharged is found to be more than balanced by that of the food.

Dr. Austin Flint, on the other hand, found that over the period of work the increase of discharge was so large that no such explanation appeared to him admissible; if, however, comparison is made of the intake with the output of nitrogen during the whole time of observation, comprising the three periods of five days each before, during, and after labour, it is found that the two are unequal, the

* "On the Elimination of Nitrogen during Rest and Exercise on a Regulated Diet," "Proc. Roy. Soc.," vol. 15, p. 339, vol. 16, p. 44, 1867; vol. 20, p. 402, 1871.

† "New York Medical Journal," June 1871, p. 609.

difference in favour of the nitrogen of the food amounting to 217 grains in a total of 5075.

These results were subjected to careful experimental criticism in 1876,* by Dr. Pavy, who showed as the result of his own analyses that the immediate effect of labour in increasing the nitrogen output of the body is more than compensated by the concomitant and subsequent intake.

It is further to be considered that whatever results had been obtained by Dr. Austin Flint, they could not have been received without some misgiving, for his methods of research were insufficient as bases for quantitative statement; thus the nitrogen of the urine was throughout determined by a process which is known to be liable to errors of variable amount, which no care on the part of the worker is adequate to guard against. So also as regards the intake of nitrogen. Dr. Flint's estimates were for the most part founded, not on analyses of the material actually used, but on calculations based on the percentages given in M. Payen's tables,† percentages which are known to be at best only approximately correct; moreover, the diet of Weston was of so complicated and variable a composition, that even if each constituent had been analysed, the result would still have been open to question.

The circumstances under which Dr. Austin Flint had to make his observations probably made it impossible for him even to attempt to secure uniformity of diet. In this respect the conditions of Dr. Parkes' experiments were immeasurably superior; fully recognising that uniformity was essential, he fed his men in the simplest possible way. He was not, however, able to accomplish this without employing a diet which was so different from that to which as soldiers they were accustomed that, however satisfactory its elementary composition might be, it could scarcely be considered as natural.

Notwithstanding this difficulty, the experiments of Dr. Parkes render it, to say the least, highly probable that the immediate effect of labour is to increase the discharge of nitrogen, they leave it undecided whether or not this increase occurs at the expense of stored material independent of any concomitant or subsequent increase of intake.

If this be so, the results of experiments conducted under natural conditions and by perfectly accurate methods, ought to show that if the nitrogen output were graphically represented during the course of it by a curve, in the construction of which the quantity of nitrogen in the food was taken as base line or axis, the part of such output curve corresponding to the previous period would be a more or less horizontal line below the level of the axis. That corresponding to the period during or immediately following work will show an elevation, followed by a depression of less amplitude but longer duration, corresponding

* "Lancet," 1876, vols. i and ii.

† "Traité des Substances Alimentaires," Paris, 1865.

to the subsequent period, and the form of the curve would be such that of the two spaces included between it and the axis, the one above would never exceed the one below, provided the period of observation were sufficiently prolonged. It has been a primary object of the present inquiry to determine whether a result in this form can be invariably obtained or not; its value, if obtained, would lie, not of course in its affording evidence that increased expenditure of nitrogen, consequent on work, is balanced by diminished expenditure afterwards (for this has been already shown, and has in itself very little significance), but in its proving that the increase is not and cannot be an effect of increased intake, that the over-expenditure of nitrogenous material which work determines is exclusively expenditure of previously stored material, and consequently that the degree of effect produced by work will vary according to the state of the body in respect of storage at the time at which the work is done.

The subject-matter of the present paper will for the sake of clearness be discussed under the following heads :—

(1.) Plan of Experiment.

(2.) Observations on Pulse, Temperature, Respiration, and Body-Weight, before, during, and after work.

(3.) Experimental Diet.

(4.) Methods of Collecting Excreta.

(5.) Methods of Analysis of Food and Excreta.

(6.) Nature of the Work done.

(7.) Detailed Statement and General Summary of the Quantitative Results of Experiments.

In the concluding paragraphs an attempt will be made to set forth the inferences to which the experiments lead, and to indicate the direction which must be given to further inquiry.

The Plan of Experiment.

I always began an experiment with the intention that it should extend over at least nine days, which I divided as follows :—Four days' ordinary occupation, one day work, and a second period of four days' ordinary occupation, or more if I thought it necessary for the complete subsidence of any effects produced by the work. In addition to this I usually prepared myself some days (ten or twelve) before, by living exceedingly regularly, and always having the same kind, and as nearly as possible, without weighing, the same quantity of food; further, in order to get rid of what in a former paper* I have called reserve nitrogen, I have found it necessary somewhat to increase my daily exercise for a few days before commencing an experiment, or, as in my later investigations, to take the additional precaution of

* "Journal of Physiology," vol. i.

abstaining from food for thirty-six hours. Judged by the results, this seems to bring about the desired effect more rapidly and with greater certainty than increased exercise, and has the advantage of not causing such great systemic disturbance—at least in my own case.

In the paper above referred to I have stated my belief as the result of experiment in the possibility of storing in the body a certain amount of nitrogen, which, when occasion arises, is discharged again, and I found in these experiments that my mode of life, and particularly the amount of food which I had been taking in the previous week, materially influenced the effect produced by the inanition: that is to say, that in the one case, in which I had not been living up to my usual standard of diet for some little time before the fast commenced, the nitrogen curve began to descend immediately and very rapidly to its minimum; whereas in the second, in which the reverse condition pertained, that is to say, when for a week before the experiment I had been living somewhat more generously than usual, the nitrogen curve descended more slowly, and the general effect of the starvation upon me was very much less; I would lay particular stress upon these facts, although beyond the use of the term "store nitrogen," I have attempted no explanation of them, because they will be found to have a very important bearing upon the results of the experiments which form the subject of this paper.

Having thus, in one way or the other, *i.e.*, either by starvation or increased exercise and regulated diet, prepared myself, I put myself upon the special experimental diet, going about my work, and living just as usual, except that my meal-times and the quantity of food taken daily were regulated with great exactness.

In the earlier experiments I performed the whole of the analyses myself, and in all I have personally weighed, prepared, and cooked every article of my diet.

The analyses of food, urine, and fæces, made in 1882 and 1883, were made for me by a skilled assistant, and I may take this opportunity to remark, that with my present mode of experiment, the labour involved is so great as to render it quite impossible for any one to be at the same time the subject of the experiment and the analyst, apart from the fact that for many reasons it is very undesirable.

Observations on Pulse, Temperature, Respiration, and Body-Weight.

These were made regularly twice a day, immediately on waking in the morning, and just before going to bed at night.

The Pulse.—The observations were made in two positions, standing and lying, the difference between the two being, I think, a fair index of the condition of the nervous system. The morning observation was made immediately on waking, and before any movements had

been made which might excite the heart. The standing observation was made by getting out of bed as quietly as possible, and standing upright for two or three minutes, during which I counted my pulse, until I found that the quickening produced by the movement had subsided. I then made the observation, often repeating it two or three times. At night I remained standing for five or six minutes before counting the pulse, and on lying down the same precaution was observed.

Respiration.—Observations in the two positions were also made morning and evening, the same care being taken to ensure accuracy.

I found it was not by any means an easy matter to count the respiration without affecting its rate by the very act of doing so. The plan eventually adopted was to stand with my watch in my left hand, and my left wrist lightly pressing against the lower ribs; with a little practice I was able in this way to count the respirations almost unconsciously. The figures given in the tables are as a rule the mean of at least two observations.

Temperature.—This was observed by means of a pair of clinical thermometers, which had been carefully compared, and found to read accurately together. The figures all indicate Fahrenheit degrees, and are something less than one-tenth of a degree too high; this being the error of the instruments, no readings were taken under five minutes, and most of them were over ten. I previously found by careful experiment that three minutes would suffice for either of the thermometers to attain the maximum temperature.

The observations were taken in the mouth and axilla, great care being taken to secure the proper position of the instruments; any doubtful observations were always repeated. I occasionally also took the temperature of the rectum. Besides the morning and evening observations, I took the temperature of the mouth frequently during the exercise and at other times, when for some reason or another I have thought it desirable to do so. The exact times at which all these observations were made, together with notes on any which seemed to require explanation, will be found recorded in the tables.

The Body-Weight.—This was taken morning and evening on an Avery's machine, weighing to ounces. On the days of exercise I made two additional observations, one just before starting, and another on returning.

There are several points connected with the accurate determination of the body-weight which are of very great importance in all investigations of this kind, and which may not be out of place here.

There is of course no difficulty whatever in determining the body-weight at any given time, but I have learned from long experience that it is not so easy to make what I may call a "fair" observation. It will be obvious from an inspection of the tables that the variations

of my own body-weight were often very considerable, and this is especially the case between the evening weight and that of the following morning; variations during the day may be very largely due to food and drink, particularly the latter. This loss of weight during sleep is due to two causes, loss by the skin and lungs, and loss by the kidneys, as I always emptied the bladder before taking my weight. Making all allowance for the urine, it is clear that whilst sleeping I frequently lost 1 to 1½ lbs. in weight, and on some occasions even more, by the skin and lungs alone. During the day I have frequently observed even greater loss from this cause.

The object of frequent and regular observations of the body-weight in all investigations of this kind is to determine whether or no there exists a relation between loss of tissue and discharge of nitrogen. The weight of proteïd which would represent the greatest increase in the nitrogen discharge I have ever observed is very small compared with the variation of body-weight from other causes, and is very liable to be overshadowed by them, if indeed we ought to consider ourselves in any degree capable of distinguishing between the two.

There are a number of circumstances which affect the body-weight, and which I think it would be well briefly to consider. They may be divided roughly into two categories, external and internal.

External Conditions.—The condition of the atmosphere, height of the barometer, external temperature, and the nature of one's occupation.

It is a matter of common observation that the amount of sensible perspiration varies very materially with meteorological conditions, and I have good reason for believing that not merely the sensible perspiration is affected, but also indirectly the total loss through the skin.

Speaking for myself, and I have often heard the same remark made by others accustomed to severe physical exertion, a low barometer and the depressing conditions which so often accompany it profoundly influence the effect of such exertion upon the body-weight; I can only suppose that this is due to the mental effect produced reacting on the vasomotor system. Nor is this all. I think that there is evidence in the results of my experiments, that this effect manifests itself in another way,* which unfortunately seriously complicates the problem I have set before me for solution. This vasomotor action affects the blood supply not merely of the skin, but also of the internal organs, and especially the kidneys, causing not only a great increase in the quantity of urine, but also an increase of excretory activity, which has for result an incontestable increase in the amount of nitrogen in the urine. This makes it exceedingly important in estimating the value of results obtained to take very

* E. Smith: "Health and Disease," 1875, p. 157.

carefully into consideration all these conditions which directly or indirectly affect not only the quantity, but the quality of the urine.

Internal Conditions.—These are very various, and hardly admit of accurate scientific description. There is, however, a phrase the meaning of which is well known to athletes, viz., "to feel fit," which will serve to illustrate my meaning. I have observed repeatedly that when I have undergone any considerable physical exertion for which at the outset I did not feel this "fitness," or, to use another phrase, when I was "out of condition," there was invariably a much greater loss of weight than when I was "in good condition."

No doubt atmospheric conditions have a very powerful influence for or against this "fitness" for exertion, but independently of these are mental and nervous states probably exceedingly complicated in their origin, but none the less powerful in the influence they have upon the metabolic phenomena of the body.

The body-weight in these experiments was always taken after emptying the bladder, so that the difference between the evening weight and the weight next morning expresses the total loss by skin and kidneys. I attempted to eliminate the error caused by the contents of the rectum, but very soon found that it was impossible to regulate and control the bowels with the same precision with which the urine can be regulated; so that if we wish very accurately to determine the significance of these differences of body-weight, we must take careful note of the times at which the fæces were passed, but for reasons which will be stated under the head of the fæces the difficulty is only then very partially overcome. Enough has I think been said to show how exceedingly difficult it may be to determine with even moderate accuracy the nature of the loss or gain in body-weight, and how careful we should be in accepting arguments founded on small variations.

Daily Occupation, Meals, &c.—The day generally began about 9 A.M.; the variations from this time will be found in the tables of temperature. Breakfast followed as soon as I could prepare it. I had no mid-day meal, but was accustomed to take a portion of the bread to the laboratory at University College, in which most of my time was spent. I dined at or about 6 P.M., and spent the evening in reading, weighing and preparing food, &c. The necessities of my work sometimes kept me up very late. This was unavoidable, as I was not able to devote my whole time to these experiments. At first I used to bake my bread every evening for the following day; but in the later experiments I gave up every alternate evening to this work, as I found I saved time by the arrangement. Supper was taken at various times after 11 P.M.

Arrangements on Days of Exercise.—Under ordinary circumstances, that is to say, when the walk could be easily accomplished between

the breakfast and dinner hour, or at the most an hour or so later, the exercise did not entail any material alteration in the arrangements already described; but when the distance to be walked exceeded 30 miles, the time required for its performance was too great to admit of even approximate adherence to the rules as to meal-times, &c., described in the previous section. For instance, on the occasion when the distance to be walked was close upon 50 miles, it was necessary to go some little distance by train as usual, in order to get out of London before commencing the march. This involved getting up at 4 A.M., breakfasting before I started, and walking to a railway station to catch a train, so that I might be on my road by 6 A.M. I did not return perhaps until 8 P.M. or even later, my sole food during the journey being one-half of my daily allowance of bread.

Before starting I had the following operations to perform:—Observations on pulse, temperature and body-weight naked, and body-weight when fully equipped for the journey, in order that I might be able to estimate the load carried. After this I had to prepare and eat my breakfast and put on my kit again before I could start. On returning my first duty was to weigh myself "all standing" and then naked; the differences between these observations and those taken in the morning gave me the actual loss of weight sustained and the mean load carried, because, as I need hardly point out, the load brought back is somewhat differently constituted from that taken out in the morning, being lighter by the weight of bread consumed and heavier by the urine and fæces brought back.

Having made these observations on my body-weight, I took my pulse, temperature, and respiration, at all events in the standing position, had a cold bath, after which I sometimes made further observations on pulse and temperature, and then prepared my dinner, of which by this time I was generally greatly in need. On the days of exercise I always combined dinner and supper, so that at the latest the last ingestion of food for the day was certainly not later than usual, and often an hour or two earlier.

During the actual progress of the walk, I frequently observed my pulse, temperature, respiration, the length of my stride, and average pace.

I have thought it necessary to state thus explicitly my manner of proceeding on a day of exercise, because it may possibly be urged that the results I have obtained may be thus accounted for; that is to say, it may be urged that whereas an ordinary day was one of twenty-four hours, the day of exercise was on two occasions at all events longer by four hours, and the day before the exercise was shortened by the same amount. I am obliged to admit a certain amount of justice in this objection, but I would urge, on the other

hand, that I was careful always to dine rather earlier on the day before the exercise, and to consume all my remaining bread at that meal, leaving only a cup of cocoa to be taken later to complete the day's allowance, and this I generally took between 8 and 9 P.M., instead of at 11 P.M. as on other days; so that I think I may safely say that my food had at least the usual time for digestion and the products to pass into the urine.

It may possibly be asked, why I did not adhere to my ordinary meal times and arrange for the consumption of my dinner on the road? I would reply, that the object of these experiments was to determine the effect of muscular labour upon the nitrogen contained in the body, and not the disposition of material supplied to the organism whilst in a state of great activity. This I consider to be a different problem from the one actually before us, and which will require investigation by somewhat different methods from those which I have adopted in these researches. True I have found it necessary to eat something during the work, but this was only the same as was eaten on other days on which no work was done.

The Diet.—In all experiments of this kind, the diet is of prime importance. Our ordinary food-stuffs do not lend themselves readily to analysis, are very variable in their composition, and are in many respects very unsuited to the purpose.

The conditions which the diet for such researches must satisfy are these:—

(1.) It must be of such a nature as to resemble as nearly as possible the ordinary food of the individual experimented upon.

(2.) It must admit of being accurately sampled for analysis.

(3.) It must keep for such a length of time that one experiment at least can be made with the same material.

The ordinary components of a mixed diet do not satisfy the last two conditions in any sense whatsoever, and the extreme difficulty of determining the ingesta accurately led me, after many trials, to use special methods of preparing the following articles for the purposes of these researches:—

Bread.
Meat.
Green vegetables.
Potatoes.
Milk.
Fat.
Apples.
Cocoa.

Bread.—In order to secure perfect uniformity in the quantity and quality of this article, I took a sufficiency of one sample of flour to

last for several experiments, and this was weighed out into paper bags in quantities sufficient for each day's consumption, an allowance of 10 grams on each day's quantity being made for loss in the process of making. This figure was arrived at by actual experiment. It will be obvious that by this method, the bread being always made in the same way and from precisely the same quantities of the same ingredients, analysis of the flour was sufficient to determine the daily quantity of each constituent, the weight of the bread itself being quite immaterial. I may add that it was raised by means of bicarbonate of soda and tartaric acid, and baked in tins to avoid possible loss in the process, and that the greatest care was taken to ensure the consumption of the whole daily quantity by keeping it in paper bags, so that no crumbs were lost.

In this way I secured absolute uniformity as regards bread.

Meat.—The difficulties to be overcome in order that the meat might be made to fulfil the conditions already laid down were much more serious than in the case of bread. They were, however, successfully met by my friend Mr. Stephen Darby, of Leadenhall Street, to whom I would here express my great obligation for the service he has rendered me in this matter. The process of preparation is briefly this:—

A large quantity, 80 to 100 lbs. of selected lean beefsteak, was carefully freed from all obvious fat, tendon, &c., cut into pieces about the size of a Barcelona nut, and dried upon a steam floor for some time, then cut into still smaller pieces and dried again, and the process repeated until the material was dry enough to be finely ground in a mill. The resulting powder was exceedingly dry, of about the consistency of building sand, practically insoluble in water even after very prolonged boiling, but readily attacked by very dilute hydrochloric acid (0·2 per cent.), and apparently very easy of digestion.

I found that the insolubility of the material somewhat interfered with the palatability of the soup made from it (in which form it was always taken but by the addition of vegetables and seasoning). This difficulty was surmounted, as was also another, viz., that the powder would not remain in suspension unless something were added to thicken the soup. It will be obvious that this meat powder fulfils the required condition almost perfectly. It will keep almost indefinitely (I have used some recently which was made nearly four years ago). It is of uniform composition, and can be very readily analysed —one analysis therefore sufficing for a very large number of experiments.

Potatoes.—There is no difficulty as to these, "Edwards' patent desiccated potato" fulfilling every condition perfectly.

Green Vegetables.—These were for a long time a source of very great trouble. At first I used ordinary fresh vegetable, which was

very unsatisfactory. Nearly all the experiments recorded in this paper have been made with what is known as "dried julienne," which, though in many respects greatly superior to fresh vegetable for the purpose of these inquiries, in that it is possible to sample and analyse it, fairly is open to the objection that it requires prolonged soaking and careful cooking, otherwise it is exceedingly indigestible.

During the progress of the last experiment, Mr. Darby prepared for me, at my suggestion, a quantity of vegetables of various kinds, by thorough cooking, draining away the water, carefully drying the residue, and reducing it to a very fine powder. The product is superior to the dry julienne in many respects. It is already cooked; it possesses much more flavour, and being in a fine powder, is very easily digested. I intend to use vegetables so prepared in all future experiments.

Milk.—This presented no difficulty. I was informed by the makers that a "case" of condensed milk was invariably the product of one operation, and in consequence the various tins in it would contain milk of similar composition. Actual analysis showed this to be the case.

The milk was weighed in small tared beakers (50 grams in each), the whole of the contents of a tin being weighed out at the same time, in order that no change by evaporation might take place in the composition of the milk. When used, after scraping out as much as possible with a spoon, I boiled the beaker in the water to be used for making the cocoa, so that nothing was lost.

Cocoa.—I used Van Houten's cocoa, because a small quantity suffices, and when boiled with the condensed milk, diluted with water, the "suspension" is perfect enough to enable the whole to be consumed without loss. In this respect it is far superior for the purpose to tea or coffee.

Fruit.—I found throughout these experiments that fruit, in some form or another, was almost a necessity and greatly contributed to my comfort. I used for this purpose "American evaporated apples," which contain no waste, are easily cooked, and exceedingly palatable.

Fats.—Prepared "Australian beef marrow," as sold in the shops, supplied the fat I required; it contained no nitrogen.

The other articles used, sugar, salt, tartaric acid, and carbonate of soda (used for raising the bread), presented no difficulties whatsoever.

Composition of the Diet.

Flour		410 grams.
Salt / Tartaric acid / Carbonate of soda	of each	7 ,,

Condensed milk	100 grams.
Fat	100 "
Meat powder	60 "
Preserved potatoes	60 "
Dried julienne	20 "
Salt (for soup and potatoes).	10 "
Cocoa	10 "
Preserved apples...........	20 "
Sugar (powdered loaf)	15 "

The distribution of the above articles into meals was as follows:—

Breakfast.

Cocoa	5 grams.
Condensed milk	50 "
Fat	50 "

Bread, about $\frac{2}{5}$ of the whole amount.

Luncheon.

Bread, about $\frac{1}{5}$ of the whole amount.

Dinner.

Meat powder	60 grams.
Julienne	20 "
Potatoes	60 "
Apples }	20 "
Sugar }	15 "
Salt.......................	5 "
Fat, about	25 "

Bread, about $\frac{1}{5}$ of the whole amount.

Supper.

Cocoa	5 grams.
Condensed milk.............	50 "
Bread and fat	The remainder.

Percentage Composition of Nitrogenous Food-stuffs used in Experiments, 1882.

Food-stuff.	Nitrogen.	P_2O_5.	H_2SO_4.	NaCl.	Ash.
Flour............	1·700	0·340	0·160	..	0·672
Dried meat	13·552	1·892	0·630	0·259	4·310
Condensed milk	1·474	0·706	0·035	0·439	2·110
Julienne	1·725	0·811	0·445	0·289	5·382
Potato..................	0·961	0·492	0·210	0·511	3·412
Cocoa	3·162	2·122	..	0·058	9·180

Composition of Daily Ingesta.

Food-stuff.	Quantity.	Nitrogen.	P_2O_5.	H_2SO_4.	NaCl.
	grams.				
Flour..................	400	6·800	1·361	0·067	
Dried meat	60	8·131	1·135	0·037	0·155
Condensed milk	100	1·474	0·706	0·355	0·439
Julienne	20	0·345	0·162	0·089	0·057
Potato.................	60	0·576	0·295	0·126	0·306
Cocoa..................	10	0·316	0·212	..	0·005
Totals	..	17·643	3·872	0·675	0·965

Preparation of Foods.—My method of preparing the soup, the chief nitrogenous food of the day, was as follows:—20 grams of julienne were weighed out the night before, and set aside in a dish, and covered with about 250 c.c. of water to soak. By 6 P.M. on the following day it was sufficiently soaked to be ready for cooking, and was then poured into a saucepan, care being taken that none either of the vegetable or water was lost. To this was added salt and dried meat (60 grams), and the whole boiled for twenty minutes or half-an-hour, and kept constantly stirred, so that nothing should adhere to the sides of the saucepan, get burned, and so be lost. When sufficiently cooked it was poured into the basin from which it was to be eaten, and the saucepan carefully cleaned with a little boiling water, which was then added to the soup. By these precautions I found no difficulty in securing the consumption of the whole of the material.

The potatoes were cooked according to the directions given by the makers, a small portion of fat being added. I found that when properly cooked there was not the slightest difficulty in removing them from the saucepan completely.

The apples were stewed in the dish with the water in which they were soaked, together with the 15 grams of sugar, and from this dish they were eaten, so that nothing was lost.

Remarks on some Considerations involved in the above Diet.

My diet during these experiments was compared with my ordinary food, a very concentrated one, and on this ground alone it has been suggested to me that it was very objectionable, and could not nourish me in the same way as my ordinary food. To this I can only reply that during the whole course of these experiments I enjoyed exceedingly good health, and provided I was careful about my diet for a few days after the conclusion of an experiment, I experienced no ill effects whatsoever; on the contrary I have found on several occasions that my general health was decidedly benefited by adopting

the diet, not for experimental but entirely personal reasons, and I have often done so. All that has been done to any of the articles which comprise my diet table is to remove the great bulk of the water, and that in such a way as to produce no alteration in the food-stuff, except in form. I think the figures prove beyond a doubt that the food was exceedingly well digested. The only fault I have to find with it is that there is so little indigestible material in it, that it frequently caused slight constipation and irregularity of the bowels, which somewhat interfered with the exactness of the experiment. The sensations experienced after a meal of dried meat and vegetables are, I think, worth recording.

Immediately after the meal there was a sense of satisfaction; this was replaced in about an hour by a sense of hunger, sometimes very marked; and this gradually disappeared and gave place to the consciousness that an amply sufficient meal had been taken.

The Fluid Food.—Nothing in my experience is so difficult to regulate as the supply of fluids to the body during experiments of this kind. Variations of external temperature, and a number of minor circumstances over which we have little or no control, lead to the consumption of drinks of various kinds at irregular times, which no doubt have a very considerable effect upon the constitution of the urine, if not upon the general metabolism of the body. No doubt the strict regulation of the fluids consumed would tend in some degree to increase the accuracy of the experiment, but it would at the same time cause an amount of discomfort to the individual experimented upon, which would in many cases, I think, counterbalance the supposed gain in accuracy.

Without any special effort on my part, I think I may say that my fluid ingesta regulated themselves. The water used for making the cocoa or the soup was practically a constant determined by the size of the cooking vessels used. Besides the fluid thus taken, I think I shall be near the truth if I say that on ordinary days a pint of water was drunk. (No alcohol was taken in any form during these experiments, and though I habitually take very little, it was my custom to abstain entirely for several days before commencing an experiment.)

On the days of exercise there was undoubtedly an increase in the fluids ingested, for I allowed myself three pints of soda-water during the day, and on more than one occasion I have suffered considerable distress from the apparent insufficiency of this allowance.

It is an accepted fact that under ordinary circumstances the ingestion of large quantities of fluid causes an increase in the nitrogen eliminated by the urine, and possibly some of the increase which I have observed on days of exercise may be due to this cause; but considering the vast increase in the loss by the skin under such circumstances, I am inclined to regard the effect to be attributed to this

cause as comparatively insignificant, and to consider the extra fluid so taken as only serving to maintain the normal balance of fluids in the body.

Method of Collecting the Excreta.

My unit of time, as before stated, was a period of twenty-four hours, commencing immediately before breakfast each day; the urine passed in this period was therefore collected and mixed. For the collection of the urine I used a pair of 40-oz. wide-mouthed stoppered bottles, which for convenience of carriage, and in order that I might have them always with me, were placed in a basket having a partition, and the lid of which was secured by a padlock, to prevent interference with the contents by other than myself or my assistant.

For the collection of the fæces I used flat-bottomed Bohemian glass basins, about 2 inches deep, and 6 inches in diameter, with flat plates of glass for covers. These fitted into wooden boxes sufficiently stout to protect the contents from breakage in travelling, the lids being made to fit very closely, and secured almost hermetically by means of four thumbscrews. The dishes and covers were all weighed most carefully, and the weight scratched upon them with a diamond, and the cases were made to fit so tightly that they might be carried in any position without risk of losing the contents; these were used for carrying the fæces from my own rooms to the laboratory, and also when on the march.

Mode of Collecting during Exercise.

As the exercise consisted of long pedestrian expeditions into the country, I was obliged to adopt some means of carrying my receptacles with me. The plan first used was to pack the urine bottles in felt jackets, to prevent breakage in an ordinary tourist's knapsack, the stoppers being carefully greased, and tied in, the fæces box I strapped to a belt at my waist. But this arrangement I very soon found to be both unsafe and very uncomfortable, and I substituted an ordinary military valise, in which I carried the bottles inclosed in thick felt jackets as before. The fæces box being strapped in the place in which the soldier would carry his canteen, a haversack for my bread, and a pair of thermometers carried in separate special pockets, so as to be accessible at once, completed my equipment.

I may remark in passing that the question of a comfortable knapsack is a very important one, as anything which causes discomfort may very seriously increase the internal, if not the external work of the body. I have tried almost every form of knapsack, and have found the modern military valise far superior to any other form. The position of the load is such that the strain is equally distributed, and there is no galling or sense of local weariness on taking it off after a long day's march, and above all it leaves the respiration perfectly

free, a quality possessed by no shoulder knapsack with which I am acquainted.

The urine and fæces of the previous day were carried to the laboratory every morning by myself, and were analysed after the following methods.

Methods of Analysis of Urine and Fæces.

The urine was passed directly into the wide-mouthed bottles already described, and after the specific gravity had been taken, was poured into a large measuring cylinder, and the amount noted. Distilled water was then added, in order to bring the quantity to a round number of cubic centimeters, *e.g.*, if the original quantity was 1320 c.c., water would be added to make it up to 1400 c.c. After the whole had been thoroughly mixed, the analysis was proceeded with.

This plan was adopted to avoid the risk of arithmetical errors in the subsequent calculation of the results of the analyses.

Nitrogen.—This was estimated by combustion with soda-lime, after the Munich method. 10 c.c. of urine were taken and evaporated with a small quantity of well-burned Calais sand and a trace of oxalic acid, over a water-bath at the boiling temperature, in exceedingly thin hemispherical glass dishes. When thoroughly dry the dish and its contents were pulverised in a mortar—all precautions being taken to prevent loss—mixed with soda-lime, and the analysis proceeded with in the usual way. The estimations were made by titration with cochineal as indicator.

Instead of glass tubes I have used wrought-iron ones, carefully tested under hydraulic pressure up to 300 lbs. on the inch.

Objection has been made to the use of iron tubes, on the ground that when hot there is a danger of their leaking, and that the corks burn. I have found these difficulties to be more imaginary than real, and with a little practice and the use of ordinary care I have obtained the most excellent results. Recently I have made a number of experiments, in which the urine, instead of being evaporated with oxalic acid as above described, was introduced directly by means of a pipette into a tube already charged with 2 or 3 inches of soda-lime, and a small quantity of cane-sugar at the end. This latter method ultimately proved equally satisfactory with the former, but it requires great caution and careful watching during the process of heating, to prevent the sudden development of steam in quantity sufficient to blow the acid out of the bulbs; but this accident can be avoided without very great difficulty. I have found it desirable, particularly when the latter method is used, to employ bulbs considerably larger than those generally sold for the purpose, and to have a small additional bulb blown, from which the point takes its origin. This is a great safeguard against the effects of possible splashing.

Chlorides.—None of the special methods in use for the estimation of chlorides in urine are satisfactory. Direct titration with silver, with potassic chromate as indicator, is practically worthless, and the time required and the risks involved in the method of estimation in the ash render it impracticable when time is of any consequence. I have therefore adopted the simpler plan of diluting the urine with 5 or 6 volumes of water, and precipitating with argentic nitrate in the presence of a minimum of nitric acid, and washing and weighing the chloride formed. The results when the operation is carefully conducted are most satisfactory.

Sulphates.—These were estimated gravimetrically in the usual way by precipitation from the boiling urine with barium chloride and a minimum of hydrochloric acid.

Phosphates.—The ordinary uranium process was used, and I need only remark of it, the absolute necessity which exists for the use of perfectly fresh ferrocyanide solution. In order to secure this I find it best to keep a quantity of the finely powdered crystals, and dissolve them as required on the testing tile.

I have made no attempt to estimate any of the individual nitrogenous bodies in the urine, but have confined myself strictly to the consideration of the total nitrogen, and the variations which it undergoes under various circumstances, without complicating the problem by attempting to discriminate between the various bodies which go to make up the total which, as a matter of fact, our present methods will not allow us to do with enough accuracy to make the investigation worth the immense amount of additional labour which it would involve.

Methods Used in the Analysis of Fæces.

The collecting apparatus has been already described, and I need only now say of it that it was thoroughly satisfactory in every respect.

On arrival at the laboratory the cases are opened, the glass dish and cover removed, and together with their contents carefully weighed, the difference between the total weight and the weights marked on the dish and cover giving the weight of the fæces. The cover was then removed, and a sufficiency of methylated spirit poured upon them to cover them. This completely destroyed all objectionable smell, and rendered the material safe from decomposition.

I did not estimate the nitrogen in each day's fæces, but divided an experiment into three periods: the period before the work, the period of the work, and the period after the work. Each sample as passed was placed with the alcohol in a large porcelain dish over the water-bath, and after being well broken up with a glass paddle, was rapidly dried, and the residue by constant stirring reduced to a dry granular

mass, which was occasionally removed very carefully to a mortar and broken up, in order to hasten the process. The products were separated into three lots, as above stated, and stored in a well stoppered bottle till the analysis could be made. The total amount of dry matter yielded by the fæces was determined, and the calculation of the analyses made from this.

The nitrogen was estimated by the soda-lime process, the material being reduced to an exceedingly fine powder, and burned in an iron tube 3 inches longer than those used for the urine, in order to secure the complete decomposition of any undigested albuminoïds which might be present.

Phosphates were estimated after partial incineration in a platinum capsule by the magnesia method, and weighed as pyrophosphate.

Chlorides.—The precipitate yielded on addition of a few drops of argentic nitrate to a watery extract of the dry material rarely amounted to more than a perceptible cloudiness, and consequently no figures appear in the analyses.

Sulphates.—The same is still more true of the precipitate yielded by the addition of barium nitrate and hydrochloric acid to the same solution. It was often an open question whether there was any precipitate at all, so small was the quantity of soluble sulphate present. This led me to consider in what form the sulphur might exist, and in the light of Lepine's experiments on the sulphur in the urine, I was led to try the effect of powerful oxidising agents on the sulphur compounds which might be present, and I very soon found that nothing short of combustion in a stream of oxygen would completely decompose them. The consequence of this was that after a series of experiments I found myself unable to devise a method by which the combustion could be accurately carried out, and at the same time with sufficient rapidity to enable the work to be completed in the time at my disposal. I regret exceedingly that this should have been the case, as the results of complete determination of the sulphur, both in the urine and fæces, would have been most interesting.

Ash.—In the process of incinerating the fæces for the estimation of the phosphates, the ash was always roughly estimated, and so regularly did the phosphates vary with the amount of ash, that as soon as this was known I was able to make a very fair estimate of the quantity present.

When I began these researches I had the intention of determining accurately the composition of each day's fæces, but it soon became obvious that the results could be of but little value, and might possibly be very misleading, from the extreme difficulty of deciding how far those passed on a given day belong to the day before, or contain something which should be credited to an earlier day, or even to the very day on which they were passed. I have attempted by

various artificial means to distinguish one day from another by eating something indigestible, and attempting to use the position of this body as a line of demarcation between one day and the next, but all attempts have hitherto proved futile.

This difficulty presents itself with the greatest force at the beginning of an experiment, because from the concentrated nature of my diet, it is liable and as a matter of fact almost always at first induces slight constipation.

Now that I adopt the plan of fasting from twenty-four to thirty-six hours before commencing an experiment, this difficulty is diminished; nevertheless in one or two cases I have found that having taken a large quantity of milk on the day before starvation, in order that by the colour of the dejecta I might have some guide to their distribution and origin, that the first fæces passed after the fast, sometimes not till the second day of the experiment, would contain traces of this milk, although the bowels had been freely moved in the twenty-four hours after it was taken. I fear we must admit the impossibility of completely emptying the intestines each day, and be content with results obtained as they are throughout these experiments by the method of averages. Even when the bowels are moved regularly every day at the same hour, the amount of dry material is by no means constant, and even when it happens to be the same, the amount of nitrogen contained in each day's quantity may be very different.

Microscopic examination showed that digestion of my experimental diet was very complete, the undigested matter consisting almost entirely of fragments of vegetable fibre, and a few starch granules (generally burst). Repeated examinations of a watery extract, prepared by boiling the dry material, failed to show the presence of either starch or sugar in any appreciable quantity. On one occasion on which I suffered slightly from diarrhœa, the extract of the ash contained an almost weighable amount of chlorides, and the amount of sulphates rose to what might be called "strong traces."

The whole of the analyses of food and excreta were done at least twice, and whenever a very discordant result was obtained, a third analysis was always made; the figures given in the tables are therefore invariably the mean of at least two results, closely agreeing.

Methods used in the Analysis of Foods.

Nitrogen.—The soda-lime process was used in all cases, every care being taken to ensure complete combustion, by very slow burning and the use of extra long tubes. The great concordance of the results is, I think, a satisfactory answer to most of the objections which can be urged against the method. The following are examples:—

Sample of flour	1·515	per cent.
	1·510	"
Dried meat	11·60	"
	11·66	"
Dried meat	13·52	"
	13·58	"
Flour	1·70	"
	1·69	"
Potato	0·961	"
	0·961	"
Milk	1·46	"
	1·48	"

Phosphates were always estimated in the ash as magnesic pyrophosphate.

Sulphates were determined in the ash in the usual way. The determinations have little bearing on the results of the experiments, except in so far as they show that the sulphuric acid in the urine has its origin chiefly from sulphur taken into the body in some other form. The determination of the total sulphur in the food would have been of little use unless the total sulphur of the excreta had been determined also, and, as before stated, this was found to be quite impossible.

The Work.—Perhaps of all the considerations involved in researches of this kind, the nature of the work done presents the greatest difficulties. We are obliged to choose between certain ordinary forms of exercise and the use of machines for the purpose. Each has its advantages and disadvantages, and in all it is well nigh impossible to determine with any reasonable approach to accuracy the total amount of work done. The external work does admit of calculation within limits, but our information with regard to the internal work of the body is scanty and unreliable in the extreme, and in the present state of our knowledge all that can be done is to eliminate the difficulty by making the internal work, as far as possible, a constant. It is manifestly unfair to compare walking with the lifting of heavy weights or either with rowing or bicyling; in none of these forms of labour are the same muscles used, and it is not only possible, but very probable, that different individuals use their muscles differently in each case; nay, more, that the same individual, unless he is constantly practising, does not use exactly the same muscles on different occasions. Walking exercise is, perhaps, the least objectionable of all, and probably for this reason it has found most favour with investigators of this problem, and for this reason I have adopted it in these experiments.

It might appear that the same distance walked over the same roads,

in the same time, and under as far as possible similar conditions, would represent equal amounts of work done, but a very little experience will suffice to show that such is not the case, and the results of my own experiments will, I think, bear out this opinion. Although accustomed from childhood to this particular form of exercise, and though the actual distances walked in these experiments was almost always well within my powers, I can say with truth that, measured by my own sensations, I have never before so fully appreciated the effect of what is called "training." For this reason, I have confined myself strictly to the consideration of gross results. When the distances were short, the effect produced was exceedingly variable, and in the final experiments of 1879 and 1882, when the labour was tolerably exhausting, all that we can say is, that the effect on the discharge of nitrogen is beyond all doubt; but that it bears no sort of relation to the work done in previous experiments.

In each succeeding walk the average pace per hour increased very perceptibly, and yet I am confident that this increase of pace did not by any means involve a proportional increase of internal work; in short, I gradually "got into training." I must confess that I see no escape from this difficulty; it might possibly be eliminated by continuing the investigations over a very long period of time and taking daily a very large amount of exercise, until as it were the bodily machinery had learned to work uniformly, and then suddenly increasing it very considerably. In my own case, a daily walk of 30 miles, increased to 50 every tenth day, would express my meaning; and even if this were kept up for two months or more, I should expect the effect of the longer marches to become less and less marked. The gist of the whole matter is, that we have to deal with a machine which has a most marvellous power of accommodating itself to the work put upon it, and this introduces a factor into the investigations of which we are at present wholly ignorant; indeed, the problem is thereby rendered so complicated, that the hope of finding any explanation of the phenomena observed is somewhat distant. As regards this particular form of exercise, assuming that it is done under the most favourable conditions, the following points must be considered in interpreting the results :—

(1.) The individual who is the subject of experiment.

(2.) The distance walked.

(3.) The pace.

(4.) The load carried.

(5.) The external conditions, such as temperature, &c.

Each must be carefully worked out in turn, and a very large number of experiments made before we shall be justified in arguing from the results obtained in one case to those of another.

Nor is this all. Some men are, so to speak, much more economical

machines than others, *i.e.*, differences of weight and all else being taken into consideration. One man will do a given amount of work upon a given diet, which to another would be simply impossible. I have had ample opportunity for observing this in the training of boats' crews in Cambridge, and I was particularly struck by the complete absence of any relation between body-weight and the amount of solid food consumed when no restrictions were placed upon the diet as regards quantity.

I hope in future experiments to be able to reduce the errors introduced by the work very considerably by the use of a work machine so contrived that the external work done can be measured fairly accurately, and the internal work to some extent regulated. Absolute results cannot, I think, be hoped for, but much may be done in the way of rendering one experiment comparable with another, which at present can hardly be said to be the case.

I introduce here a series of experiments made in 1879, which, though not made with such accuracy of method as the later series of 1882 and 1883, tend very materially to confirm the results then obtained.

Experiments of 1879.

The nitrogen of the fæces in these experiments was estimated by mixing the whole quantity passed during each experiment, and making one analysis in each case. The total quantity was as follows:—

		Nitrogen.
Experiment	I	18·093 grams.
„	II	16·083 „
„	III	15·294 „
„	IV	16·872 „

Diet during the Experiments of 1879.

Food-stuff.	Daily quantity.	Nitrogen.
	grams.	
Coarse oatmeal	500	9·485
Carolina rice..............	50	0·484
Dried meat................	60	6·168
Condensed milk...........	100	1·330
Fresh potatoes.............	300	0·990
Fresh apples...............	300	0·000
Beef fat	100	0·000
Van Houten's cocoa	12	0·400
	1422	18·857

The oatmeal was made into thin cakes, the rice cooked in just so much water as it would absorb, and eaten with the apples. The potatoes were baked in the usual manner. The sugar and salt used in these experiments were not weighed, but taken to taste.

The nitrogen in the oatmeal, meat, rice, milk, and cocoa was determined by the soda-lime method; the nitrogen of the potatoes was calculated from Payen's tables; they were all grown on one patch of ground on the Rothamsted farm, and were presented to me by Mr. Lawes for the purposes of these experiments. Their composition was probably fairly uniform.

Nothing excepting the nitrogen was estimated in the food-stuffs, and consequently no comparison can be instituted between the ingesta and egesta except with regard to the nitrogen.

The nitrogen of the urine was estimated by Liebig's process; the chlorides by direct titration with argentic nitrate, with potassium chromate as indicator; the phosphates by the uranium process, and the sulphates by precipitation with barium nitrate.

Composition of Urine, Experiment I, 1879. March 5 to March 13.

Date.	Quantity.	Specific gravity.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen. Liebig.
	c.c.					
March 5..	895	1035	13·97	2·43	2·78	13·80
„ 6..	1173	1036	12·45	4·03	2·69	18·26
„ 7..	1266	1031	11·46	2·91	2·32	17·39
„ 8..	1433	1024	8·95	2·52	2·92	16·50
„ 9..	925	1034	12·14	2·16	2·01	17·09
„ 10	873	1032	5·81	3·14	2·89	16·80
„ 11..	980	1030	12·25	3·14	2·32	16·16
„ 12..	1255	1027	12·04	2·78	2·85	16·99
„ 13..	1440	1027	14·99	3·13	2·76	17·31
Totals ...	10240	..	104·06	26·24	23·54	150·30
Averages .	1137	1030	11·56	2·91	2·61	16·70

Composition of Urine, Experiment II, 1879. March 19 to March 26.

Date.	Quantity.	Specific gravity.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen. Liebig.
	c.c.					
March 19.	760	1035	7·60	2·26	2·67	14·01
„ 20.	1155	1030	17·77	2·55	2·55	14·51
„ 21.	1485	1026	19·48	2·52	2·64	14·51
„ 22.	2290	1017	21·46	2·50	2·63	16·08
„ 23.	2390	1020	21·89	2·68	4·18	21·42
„ 24.	970	1032	12·52	1·70	3·15	17·31
„ 25	1091	1032	17·72	3·00	2·79	16·04
„ 26	1640	1027	20·50	2·27	2·85	16·82
Totals ...	11781	..	138·94	19·48	23·46	130·70
Averages.	1472	1027	17·36	2·43	2·93	14·83

Composition of Urine.
Experiment III, 1879. April 2 to April 9.

Date.	Quantity.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen. Liebig.	Specific gravity.
	c.c.					
April 2 ..	663	5·24	2·92	2·55	13·04	1034
„ 3 ..	1675	22·67	1·58	2·99	18·23	1023
„ 4 ..	1588	19·83	1·64	2·54	15·98	1022
„ 5 ..	990	19·38	2·34	2·57	12·95	1030
„ 6 ..	2064	20·64	1·17	3·32	17·55	1020
„ 7 ..	927	14·09	3·07	2·67	15·69	1032
„ 8 ..	1400	18·95	2·01	2·47	17·54	1027
„ 9 ..	1227	18·70	2·09	2·54	14·92	1029
Totals ...	10534	139·50	16·82	21·65	125·24	
Averages .	1316	17·43	2·10	2·70	15·74	1027

Composition of Urine.
Experiment IV, 1879. May 7 to May 14.

Date.	Quantity.	Specific gravity.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen. Liebig.
	c.c.					
May 7...	695	1031	7·23	2·03	1·94	11·28
„ 8...	1500	1026	20·31	2·88	3·06	16·31
„ 9...	1480	1022	19·41	2·25	2·13	12·98
„ 10...	1800	1026	19·49	4·28	5·38	24·03
„ 11...	810	1033	12·15	3·04	3·29	17·66
„ 12...	1038	1021	8·64	3·09	3·32	18·69
„ 13...	1500	1022	16·87	2·84	2·80	16·17
„ 14...	164)	1020	18·77	2·90	2·63	15·16
Totals ...	10463	..	122·87	23·31	24·55	132·28
Averages.	1307	1025	15·35	2·91	3·06	16·33

Comparison of the Nitrogen, Phosphoric and Sulphuric Acids of the Urine before and after the Work. Experiments of 1879.

Daily Averages.

	Before work.	After work.	Differences.
No. I.—Nitrogen....	16·48	16·87	0·39
P_2O_5	2·97	2·87	0·10
H_2SO_4	2·67	2·56	0·11
No. II.—Nitrogen....	14·77	17·89	3·12
P_2O_5	2·45	2·41	0·04
H_2SO_4......	2·62	3·24	0·64
No. III.—Nitrogen....	15·05	16·42	1·37
P_2O_5	2·12	2·08	0·04
H_2SO_4......	2·66	2·75	0 09
No. IV.—Nitrogen....	13·52	18·34	4·82
P_2O_5	2·38	3·23	0·85
H_2SO_4......	2·37	3·48	1·11

Balance of Nitrogen in Ingesta and Excreta. Experiments of 1879.
Experiment I, 1879. Work done March 9, 1879.

	Urine.	Fæces.	Total.	Ingesta.
No. I.—Totals	150·30	18·09	168·39	169·71
Daily.......	16·70	2·01	18·71	18·85
Differenes ...	..	..	..	1·32
No. II.—Totals	130·70	16·08	146·78	150·85
Daily	14·83	2·01	18·34	18·85
Differences ..	..	..	..	4·07
No. III.—Totals	125·94	15·29	141·23	150·85
Daily	15·74	1 91	17·65	18·85
Differences ..	..	..	..	9·62
No. IV.—Totals	132·28	16·87	149·15	150·85
Daily	16·53	2·10	18·64	18·85
Differences ..	..	..	..	1·70

Experiment I, 1879.

Distance walked	30 miles.
Total time of journey..............	$8\frac{1}{4}$ hours.
Halts	$1\frac{1}{4}$ hour.
Actual time of walking	7 hours.
Average pace	4·28 miles per hour.
Load carried.....................	22·56 lbs.

Experiment II, 1879. Work done March 23, 1879.

Distance walked	33 miles.
Total time of journey	$8\frac{1}{4}$ hours.
Halts	$1\frac{1}{4}$ hour.
Actual time of walking	$7\frac{1}{4}$ hours.
Average pace	4·55 miles per hour.
Load carried	24 lbs.

Remarks.—Stiff north-easterly gale blowing all day; heavy rain and sleet, bitterly cold.

Experiment III, 1879. Work done April 6, 1879.

Distance walked	26 miles.
Total time of journey	6·75 hours.
Actual time of walking	6 hours.
Average pace	4·33 miles per hour.
Load carried	30·12 lbs.

Remarks.—Rained heavily and without ceasing during the whole journey.

Experiment IV, 1879. Work done May 10, 1879.

Distance walked	49 miles.
Total time of journey	12 hours.
Halts	$1\frac{1}{2}$ hour.
Actual time of walking	$10\frac{1}{2}$ hours.
Average pace	4·66 miles per hour.
Load carried	30·31 lbs.

Remarks.—Had nearly one hour's sleep in the middle of the day; the pace during the last two hours was considerably above the day's average. I returned to London by train very tired and stiff, but slept soundly and was practically recovered the next day.

Date.	Time.	Pulse, lying.	Temperature, mouth.	Body-weight.	Mean daily body-weight.
1879.					
March 5....	8 A.M.	56	98·0	129·31	
„ 6....	12.15 „	58	98·8	134·06	131·68
	9 „	54	98·0	132·56	
„ 7....	12.30 „	53	98·1	134·31	133·43
	8.30 „	56	98·3	132·37	
„ 8....	12.15 „	52	98·4	134·18	133·27
	8.30 „	49	98·4	133·25	

Date.	Time.	Pulse, lying.	Temperature, mouth.	Body-weight.	Mean daily body-weight.
1879.					
March 9....	12.15 A.M.	54	98·6	133·00	133·12
	8 ,,	54	98·2	130·87	
	7 P.M.	..	..	126·62	
	12 ,,	..	..	130·68	129·39
,, 10....	9.30 A.M.	56	98·6	129·00	
,, 11....	2 ,,	60	99·8	133·25	131·12
	9.30 ,,	56	98·4	131·12	
,, 12....	2 ,,	54	98·6	133·43	132·27
	8.30 ,,	57	98·2	131·93	
,, 13....	1.30 ,,	52	98·7	134·50	133·21
	9.30 ,,	53	98·1		
,, 14....	2 ,,	52	98·0	135·12	
	9.30 ,,	48	98·2	133·00	
,, 15....	2 ,,	58	99·3	131·37	132·18
	9 ,,	53	98·1	130·12	
,, 16....	1.30 ,,	57	99·0	131·37	130·74
	12.30 P.M.	..	..	129·37	
,, 17....	1 A.M.	60	99·0	129·56	129·46
	8.30 ,,	51	98·4	128·43	
,, 18....	1.30 ,,	57	98·7	132·25	130·34
	10 ,,	63	98·6	130·75	
,, 19....	2.30 ,,	63	99·8	129·50	130·12
	11 ,,	54	98·3	128·31	
,, 20....	12.15 ,,	58	98·9	134·56	131·43
	10 ,,	54	98·4	132·75	
,, 21....	1.30 ,,	52	98·7	134·87	133·81
	10 ,,	49	98·4	133·18	
,, 22....	12.30 ,,	..	98·7	135·50	134·34
	9 ,,	54	98·3	133·68	
,, 23....	1.30 ,,	56	99·0	133·37	133·52
	8 ,,	53	98·0	132·18	
	9 P.M.	..	..	128·18	
,, 24....	12.30 A.M.	59	98·5	131·00	130·45
	11 ,,	49	98·5	128·81	
,, 25....	2.30 ,,	49	99·0	134·00	131·40
	11 ,,	50	98·7	132·37	
,, 26....	2 ,,	56	98·8	134·68	133·52
	10 ,,	53	98·6	133·00	
,, 27....	2 ,,	59	100·8	135·00	134·00
	10 ,,	49	98·7	133·12	
,, 28....				..	
,, 29....	2 ,,	54	99·0	130·43	
	10 ,,	..	98·4	128·68	
,, 30 ...	1 ,,	57	99·5	130·75	129·71
	10 ,,	54	99·0	129·18	
,, 31....	1 ,,	51	98·0	129·12	129·15
	9 ,,	55	98·0	128·0	
April 1	2 ,,	49	98·4	129·87	128·93
	10 ,,	51	98·4	128·43	
,, 2	3 ,,	51	99·0	129·37	128·90
	9.30 ,,	53	97·9	128·68	
,, 3	1 ,,	56	98·4	134·18	131·43
	10 ,,	54	98·4	132·50	
,, 4	2 ,,	53	98·6	133·62	133·04
	10.30 ,,	59	98·4	131·81	
,, 5	1.30 ,,	..	99·8	132·43	132·12
	11 ,,	55	98·6	130·43	

Date.	Time.	Pulse, lying.	Temperature, mouth.	Body-weight.	Mean daily body-weight.
1879.					
March 6	1 A.M.	58	99·0	134·50	132·46
	9 „	..	98·0	132·75	
	10 P.M.	61	99·3	128·06	130·93
„ 7	1 A.M.	..	..	132·00	
	11 „	49	98·0	130·18	
„ 8	1 „	52	98·7	134·68	132·43
	10 „	54	98·4	133·25	
„ 9	1 „	54	98·8	135·18	134·21
	10 „	56	98·4	133·12	
„ 10	12.30 „	53	98·8	134·93	134·02
	8 „	54	98·0	133·43	
May 5......	8.30 „	50	97·8	130·37	
	11 P.M.	48	99·0	132·00	131·18
„ 6......	9 A.M.	51	98·4	130·68	
„ 7......	3 „	52	98·8	130·18	130·43
	8.30 „	49	98·2	129·31	
„ 8......	1 „	49	98·2	134·31	131·20
	9 „	48	98·2	132·50	
„ 9......	2 „	53	98·8	134·68	133·59
	10 „	55	98·4	132·18	
	11 P.M.	57	..	134·31	133·24
„ 10......	3.30 A.M.	54	98·4	133·06	
	11 P.M.	..	..	126·25	
„ 11......	2 A.M.	..	99·3	129·25	129·52
	11 „	57	98·4	127·00	
„ 12......	1 „	53	98·8	132·43	129·71
	9 „	52	98·4	130·87	
„ 13......	3 „	51	98·6	134·31	132·59
	11 „	59	98·4	132·62	
„ 14......	1 „	59	98·7	135·31	133·96
	9 „	59	98·4	133·43	
„ 15......	1 „	52	98·8	132·68	133·05
	8 „	58	98·0	131·31	

The 1882 *Series of Experiments.*

The following tables exhibit the whole of the results of the observations made during these experiments; for convenience of reference I have designated them by letters, as follows:—

Table A.—Quantity, specific gravity, and chemical composition of the urine.

Table B.—The time of passing, quantity, and composition of the fæces.

Table C.—The total daily discharge of nitrogen by urine and fæces.

This table requires a brief explanation. In order to distribute the fæces as evenly as possible over the experiment, which, as I have explained before, is very difficult to do satisfactorily, I have assumed, as I have endeavoured to show I have some right to assume, that generally speaking each quantity of fæces represents a day. On this

assumption I have set back the fæces, so that in Table C they appear opposite the day to which they probably belong. In some cases this has not been possible. On the whole I think this plan is better than the alternative one of dividing them equally over the whole time, or considering them quite separately.

Table D shows the daily state of what I may call the debit and credit account of the body as regards nitrogen. The fluctuations of the "store nitrogen" can be comprehended very readily from it.

Table E gives the daily discharge of P_2O_5 by urine and fæces, on the same assumption as that made in constructing Table C.

Table F shows the results of all observations on pulse, respiration, temperature, and body-weight.

Table G gives the mean daily value of the observations recorded in Table F.

Table H shows the amount of work done, and the loss of weight sustained in consequence.

I had originally intended to enter upon the question of the force value of the food and its relation to the energy expended during the work. At the best it would only have been an estimate based on Professor Frankland's* tables, and the data from which the work of the body, both internal and external, are so inexact and untrustworthy, that under the circumstances I have thought it best to content myself with a simple statement of the number of miles walked, and the time occupied in the performance of the work. It will be sufficient here to say that the general truth of the conclusions drawn from the classical experiment of Professors Fick and Wislicenus is confirmed by the results of these experiments in every case.

It is to be understood that *except when otherwise stated*, the diet was that given in the table accompanying the section in which the diet was discussed, and that no exercise was taken except such as was involved by my ordinary occupation.

Experiment I, 1882. *May 24th to June 1st.*

The figures in Table A point conclusively to the fact that the work done on May 28th caused a considerable increase in the nitrogen discharged by the urine. The average daily discharge on the four days before the work was 14·15 grams, whilst during the remaining five days it rose to an average of 15·74 grams, and the increase not only affected the discharge on the day of the work, but every succeeding day. There was apparently no tendency to return to the average discharge before the work, and it would seem as if the exertion had simply served to give tone and regularity to the nitrogen discharge.

* "The Origin of Muscular Power," "Phil. Mag.," No. 215, September, 1866.

The average daily elimination by urine and fæces combined is: first four days, 16·64 grams; last five days, 17·88 grams.

Despite this increased discharge, there were remaining in the body 2·77 grams of nitrogen known to have been taken in as food. The figures of Table E are interesting, as showing that up to the day of work the body was steadily accumulating nitrogen, which was as steadily given out in the period after the work; it is perhaps a matter of regret that the experiment was not prolonged for a few days, in order to observe whether the ratio of the ingesta to the excreta would have returned to that existing before the work. It is worthy of remark that though the exercise increased the discharge, the amount of nitrogen retained in the body during the period after the work was greater than before it.

The average daily discharge by the fæces was: first four days, 2·48 grams; last five days, 2·15 grams, clearly pointing to increased absorption by the intestines.

The Phosphoric Acid.—Comparison of the ingesta and excreta shows an excess in the latter of 2·60 grams. There can be, I think, but little doubt that this was due to the diet before the experiment commenced, the difference in the P_2O_5 of the ingesta and excreta for the first four days, *i.e.*, the period before the work, being 2·72 grams, after the work the daily discharge was 3·84 grams, and the daily entry 3·87 grams, or, allowing for error of experiment, practically the same, since it would appear from the consideration of the phosphoric acid, that the effect of the work was to regulate the daily discharge.

The agreement of the figures may be accidental, but considering that they are the sum of a number of separate analyses, this can hardly be the case, and this I think tends to justify the distribution of the fæces according to the plan which I have adopted.

The sulphuric acid was undoubtedly increased after the work to the extent of 0·24 gram per diem. The bulk of this excess was excreted on the day of work itself, as the following figures show:—

	Daily average.
H_2SO_4 four days before work	2·76 grams.
H_2SO_4 fours days after work	2·88 ,,
H_2SO_4 day of work	3·48 ,,

that is, an increase on the day of work of 0·66 gram above the average of the whole of the rest of the experiment.

Composition of Urine.

Experiment I, 1882. May 24 to June 1.

Table A.

Date.	Quantity.	Specific gravity.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen.
	c.c.					
May 24.	700	1022	2·88	2·43	2·78	14·80
„ 25.	800	1026	6·87	2·01	2·75	13·95
„ 26.	1000	1022	11·72	1·71	2·77	14·10
„ 27.	1100	1024	12·93	1·89	2·75	13·77
„ 28*	1000	1025	9·39	1·76	3·48	15·57
„ 29.	860	1024	7·83	2·04	2·96	15·92
„ 30.	1200	1025	11·25	1·93	2·85	15·89
„ 31.	1100	1025	13·94	2·16	2·92	15·38
June 1.	1210	1025	15·05	2·05	2·81	15·94
Totals ..	8970	..	91·86	18·00	26·07	135·33
Averages	996	1024	10·20	2·00	2·89	15·03

* Day of work.

Composition of Fæces.

Experiment I, 1882. May 24 to June 1.

Table B.

Date.	Time of passing.	Quantity.	Phosphates as P_2O_5.	Nitrogen.
		grams.		
May 24.....	rejected			
„ 25.....	..	none		
„ 26.....	4.30 P.M.	110	2·44	2·22
„ 27.....	2.30 „	127·5	2·72	2·57
„ 28.....	12.15 A.M.	130·5	2·79	2·63
„ 29.....	12 M.	145	2·23	2·52
„ 30.....	2.15 P.M.	119	1·79	2·08
„ 31.....	2 „	132	1·99	2·31
June 1.....	1 „	108	1·63	1·89
„ 2... {	12·30 „	147	2·21	2·57
	11 „	109	1·64	1·90
Totals	..	1128	19·44	20·69
Averages..	..	125·3	2·16	2·29

Total Daily Elimination by Urine and Fæces.

Experiment I, 1882. May 24 to June 1.

Table C.

Date.	Nitrogen of urine.	Nitrogen of fæces.	Totals.
May 24.......	14·80	2·22	17·02
„ 25.......	13·95	2·57	16·52
„ 26.......	14·10	2·63	16·73
„ 27.......	13·77	2·52	16·29
„ 28*......	15·57	2·08	17·65
„ 29.......	15·90	2·31	18·21
„ 30.......	15·89	1·89	17·78
„ 31......	15·38	2·57	17·95
June 1.......	15·94	1·90	17·84
Totals	135·32	20·69	156·02
Averages....	15·03	2·29	17·33

* Day of work.

Total nitrogen ingested		158·78
„ „ excreted		156·02
	Difference.....	2·76

Daily Balance of the Nitrogen of Ingesta and Excreta.

Experiment I, 1882. May 24 to June 1.

Table D.

Date.	Nitrogen of ingesta.	Nitrogen of excreta.	Difference.
May 24.......	17·64	17·02	0·62
„ 25.......	35·28	33·54	1·74
„ 26.......	52·92	50·27	2·65
„ 27.......	70·57	66·56	4·01
„ 28.......	88·21	84·21	4·00
„ 29.......	105·85	102·44	3·41
„ 30.......	123·50	120·22	3·28
„ 31.......	141·14	138·17	3·97
June 1.......	158·78	156·01	2·77

Total Daily Elimination of P_2O_5.

Experiment I, 1882. May 24 to June 1.

Table E.

Date.	P_2O_5 of urine.	P_2O_5 of fæces.	Total.
May 24.......	2·43	2·44	4·87
„ 25.......	2·01	2·72	4·73
„ 26.......	1·71	2·79	4·50
„ 27.......	1·89	2·23	4·12
„ 28*......	1·76	1·79	3·55
„ 29.......	2·04	1·99	4·03
„ 30.......	1·93	1·63	3·56
„ 31.......	2·16	2·21	4·37
June 1.......	2·05	1·64	3·69
Totals	17·98	19·44	37·44
Averages....	2·00	2·16	4·16

Total P_2O_5 ingested .... 34·84 .... daily 3·87

„ excreted .... 37·44 .... „ 4·16

Excess of P_2O_5 in excreta .... 2·60

* Day of work.

Observations on Pulse, Respiration, Temperature, and Body-Weight.

Experiment I, 1882. May 24 to June 2.

Table F.

Date.	Time.	Pulse.		Respiration.		Body-weight.	Temperature, F°.	
		L.	S.	L.	S.		Mouth.	Axilla.
						lbs.		
May 24...	10 A.M.	..	..	..	..	129·75		
„ 25...	12.15 „	69	91	..	..	132·25	..	98·0
„ 25...	9.30 „	60	71	..	..	130·50	..	97·8
„ 25...	12 P.M.	63	80	..	..	133·11	..	97·3
„ 26...	8.30 A.M.	56	78	..	..	132·00	..	97·2
„ 27...	1.15 „	68	85	..	..	133·75	..	97·4
„ 27...	9 „	53	66	..	..	132·25	..	97·2
„ 28*..	1.15 „	65	84	..	..	133·75	..	97·8
„ 28...	7.45 „	52	64	..	..	132·00	..	96·4
„ 28...	10.30 „	..	134	..	..	..	100·0	
„ 28...	10.40 „	..	144	..	..	..	100·1	
„ 28...	3.15 P.M.	..	..	..	..	..	100·0	
„ 28...	7.20 „	83†	114					

* Day of work.

† Taken whilst sitting in the train.

Table F—*continued.*

Date.	Time.	Pulse.		Respiration.		Body-weight.	Temperature, F°.	
		L.	S.	L.	S.		Mouth.	Axilla.
						lbs.		
May 28...	8.25 P.M.	..	..	..	..	129·00	99·7	
„ 28...	10 „	..	..	..	..	..	99·2	
„ 29...	1 A.M.	77	102	..	..	131·25	..	99·0
„ 29...	9.20 „	54	79	..	..	129·50	97·6	97·2
„ 30...	3 „	64	78	..	..	132·25	..	97·4
„ 30...	9 „	46	52	..	..	130·75	..	96·7
„ 31...	3 „	60	86	..	..	133·75	..	97·8
„ 31...	9 „	50	70	..	..	129·50	..	97·2
June 1...	2.30 „	59	73	..	..	133·00	..	97·4
„ 1...	9.30 „	52	54	..	..	131·25	..	97·3
„ 2...	2.30 „	58	64	..	..	132·50	..	97·4
„ 2...	10 „	54	52	..	..	131·25	..	97·4

Mean Daily Body-Weight. Pulse and Temperature.

Experiment I, 1882. May 24 to June 1.

Table G.

Date.	Body-weight.	Pulse.		Axillary temperature, F°.
		L.	S.	
	lbs.			
May 24.......	131·00			
„ 25.......	132·18	61·5	75·5	97·55
„ 26.......	132·87	62·0	81·5	97·30
„ 27.......	133·00	59·0	75·0	97·50
„ 28.......	130·75	64·5	83·0	97·70
„ 29.......	130·87	59·0	78·5	97·30
„ 30.......	132·25	53·0	69·0	97·20
„ 31.......	130·75	54·5	71·5	97·30
June 1.......	131·37	55·0	59·0	97·30
Averages....	131·67	58·5	74·1	97·40

Experiment I, 1882. May 24 to June 1.

The Work done May 28, 1882.

Table H.

Distance walked	30 miles.
Total time of journey	8 hours.
Halts	1 hour.
Actual time of walking	7 hours.
Average pace.......................	4·28 miles per hour.
Weight naked before starting........	132 lbs.
" on returning..........	129 "
Loss............................	6 "
Weight of clothes and kit, *i.e.*, the load carried	28 "

Remarks.—This was the first long walk I had taken for three months. I was out of condition. The day was exceedingly hot, and perspiration excessive.

Experiment I, 1882.

	Whole experiment.	Daily.
Nitrogen of urine	135·33	15·03
" of fæces	20·69	2·29
Total excreta	156·02	17·32
Total ingesta	158·78	17·64
Difference	2·76	0·32
P_2O_5 of urine	18·00	2·00
P_2O_5 of fæces	19·44	2·16
Total excreta	37·44	4·16
Total ingesta	34·84	3·87
Differences	2·60	0·29

	Before work.	After work.	Difference.
Nitrogen of urine	14·15	15·74	1·59
" of fæces	2·48	2·15	0·33
Total nitrogen	16·66	17·89	1·23
P_2O_5 of urine	2·01	2·00	0·01
P_2O_5 of fæces........	2·54	1·85	0·69
Total excreta	4·55	3·84	0·71
H_2SO_4 in urine	2·76	3·00	0·24

Experiment II, 1882. *June 7th to June 15th.*

The Nitrogen.—The work produced a very obvious increase on the day on which it was done, which was continued and was even more marked on the day following; the average daily discharge before the work was 15·22 grams, after the work 17·95 grams, despite this increased excretion. The reserve at the end of the experiment was only 1·54 grams less than on the day before the work; in the previous experiment the diminution in the reserve amounted to 1·23 grams.

The Phosphoric Acid.—There was an excess of phosphoric anhydride excreted over that ingested over the whole experiment amounting to 0·5 gram, a quantity probably well within the errors of experiment; still it is worthy of remark that the difference during the first four days amounted to 0·56 gram, which would seem to indicate the previous diet as an explanation.

The Sulphuric Acid.—As in the first experiment there is an undoubted increase after the work, the excess amounting to 1·42 grams over that which would have been excreted had the average of the first four days been maintained.

Composition of Urine.

Experiment II, 1882. June 7 to June 15.

Table A.

Date.	Quantity.	Sp. gr.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen.
	c.c.					
June 7..	900	1018	2·54	2·40	2·92	15·27
„ 8..	1100	1023	10·55	1·81	2·66	13·92
„ 9..	1440	1025	16·08	1·87	2·75	13·43
„ 10..	1070	1022	12·42	1·80	2·66	12·49
„ 11..	1100	1026	12·13	1·98	3·65	16·15
„ 12..	830	1020	7·58	1·86	3·02	16·31
„ 13..	1100	1025	12·47	1·89	2·55	14·98
„ 14..	1190	1025	13·78	1·80	2·88	15·46
„ 15..	1290	1023	14·36	1·66	2·79	13·59
Totals ..	10020	..	101·91	17·07	25·88	131·60
Averages	1113	1023	11·32	1·89	2·87	14·62

Composition of Fæces.

Experiment II, 1882. June 7 to June 15.

Table B.

Date.	Time of passing.	Quantity.	Phosphates as P_2O_5.	Nitrogen.
		grams.		
June 7......	..	rejected.		
„ 8......	4.30 P.M.	52	1·14	1·01
„ 9......	12.45 „	131	2·87	2·56
„ 10......	3 „	114	2·50	2·22
„ 11	..	none		
„ 12......	1 „	132·5	2·77	2·57
„ 13......	1.15 „	130	2·08	2·47
„ 14......	1 „	148	2·36	2·81
„ 15......	5 „	107	1·71	2·03
„ 16......	4.45 „	178	2·84	3·38
Totals	..	992·5	18·27	19·05
Averages	..	110·2	2·03	2·11

Total Daily Elimination of Nitrogen by Urine and Fæces.

Experiment II, 1882. June 7 to June 15.

Table C.

Date.	Nitrogen of urine.	Nitrogen of fæces.	Totals.
June 7........	15·27	1·01	16·28
„ 8........	13·92	2·56	16·48
„ 9........	13·43	2·22	15·65
„ 10........	12·49	0·00	12·49
„ 11*.......	16·15	2·57	18·72
„ 12........	16·31	2·46	18·78
„ 13........	14·98	2·81	17·79
„ 14........	15·46	2·03	17·49
„ 15........	13·59	3·38	16·97
Totals	131·60	19·05	150·65
Averages	14·62	2·11	16·74

* Day of work.

Daily Balance of the Nitrogen of Ingesta and Excreta.
Experiment II, 1882. June 7 to June 15.
Table D.

Date.	Nitrogen of ingesta.	Nitrogen of excreta.	Difference.
June 7......	17·64	16·28	1·36
„ 8......	35·28	32·76	2·52
„ 9......	52·92	48·41	4·51
„ 10......	70·57	60·90	9·67
„ 11......	88·21	79·62	8·59
„ 12......	105·85	98·40	7·45
„ 13......	123·50	116·19	7·31
„ 14......	141·14	133·68	7·46
„ 15......	158·78	150·65	8·13

Total Daily Elimination of P_2O_5.
Experiment II, 1882. June 7 to June 15.
Table E.

Date.	P_2O_5 of urine.	P_2O_5 of fæces.	Total.
June 7......	2·40	1·14	3·54
„ 8......	1·81	2·87	4·68
„ 9......	1·87	2·50	4·37
„ 10......	1·80	0·00	1·80
„ 11......	1·98	2·77	4·75
„ 12......	1·86	2·08	3·94
„ 13......	1·89	2·36	4·25
„ 14.....	1·80	1·71	3·51
„ 15......	1·66	2·84	4·50
Totals	17·07	18·27	35·34
Averages....	1·89	2·03	3·92

Total P_2O_5 excreted	35·34
„ ingested	34·84
Difference	0·50

Observations on Pulse, Respiration, Temperature, and Body-Weight.
Experiment II, 1882. June 6 to June 16.

Table F.

Date.	Time.	Pulse. L.	Pulse. S.	Respiration. L.	Respiration. S.	Body-weight.	Temperature. Mouth.	Temperature. Axilla.
						lbs.	F°.	F°.
June 6....	9.30 A.M.	56	80	..	..	129·25	..	97·0
„ 7....	1.30 „	57	76	..	..	129·37	..	97·4
„ 7....	10 „	48	58	14	15	129·75	..	97·1
„ 7....	4.10 P.M.	..	..	..	..	..	98·1	
„ 7....	11.30 „	..	..	..	17	..	98·0	
„ 8....	1.45 A.M.	62	64	16	16	134·25	..	96·8
„ 8....	10 „	56	68	13	13	132·50	..	97·6
„ 8....	6 P.M.	..	..	..	15	..	98·8	
„ 9....	2 A.M.	79	82	15	15	134·75	..	97·7
„ 9....	10 „	52	56	14	14	132·75	..	97·0
„ 9....	4.30 P.M.	..	..	..	..	..	99·0	
„ 10....	3.15 A.M.	74	84	18	18	..	..	98·0
„ 10....	9.30 „	50	54	15	15	131·62	..	97·0
„ 10....	2.15 P.M.	..	..	..	..	..	98·4	
„ 11....	1.30 A.M.	61	78	15	18	132·50	..	97·2
„ 11....	8 „	50	69	16	16	131·25	..	97·0
„ 11 ...	10.45 „	..	..	..	..	..	99·2	
„ 11....	11.15 „	..	..	..	15			
„ 11....	11.30 „	..	..	..	..	..	99·2	
„ 11....	12.25 P.M.	..	108					
„ 11....	4.20 „	..	..	..	..	..	99·2	
„ 11....	8.30 „	..	..	..	..	126·75	98·4	
„ 12....	1.30 A.M.	53	72	16	15	130·00	..	96·8
„ 12....	9.30 „	56	78	16	15	128·75	..	97·4
„ 12....	5.30 P.M.	..	..	..	..	..	98·4	
„ 13....	12.10 A.M.	..	..	..	..	..	98·7	
„ 13....	2.15 „	49	54	15	16	131·75	..	97·0
„ 13....	10.30 „	58	52	14	16	130·62	..	98·0
„ 13....	5 P.M.	..	..	..	..	..	99·0	
„ 13....	6 „	..	..	..	..	..	99 0	
„ 14....	1.30 A.M.	60	84	17	17	133·00	97·3	97·0
„ 14....	10 „	52	68	13	14	131·75	..	97·3
„ 14....	4 P.M.	..	..	..	..	..	99·1	
„ 14....	12 „	..	..	..	..	..	99·2	
„ 15....	2 A.M.	60	70	15	18	133·75		
„ 15....	11 „	54	56	13	14	131·87	..	97·6
„ 16....	2.30 „	66	84	16	17	132·50	..	97·4
„ 16....	9.30 „	..	..	..	..	131·00		

Mean Daily Body-Weight, Pulse, and Temperature.

Experiment II, 1882. June 6 to June 15.

Table G.

Date.	Body-weight.	Pulse.		Respiration.		Axillary temperature.
		L.	S.	L.	S.	
	lbs.					F°.
June 6........	130·31	56·5	78·0	..	..	97·20
„ 7........	131·90	55·0	61·0	14·5	15·5	96·95
„ 8........	133·67	67·5	75·0	14·2	15·0	97·65
„ 9........	133·00	63·0	70·0	16 0	16·0	97·50
„ 10........	132·06	55·5	66·0	15·0	16·5	97·10
„ 11........	129·25	51·5	70·5	16·0	15·5	96·90
„ 12.... ..	130·25	52·5	66 0	15·7	15·5	97·20
„ 13........	131 81	59·0	66·0	15·5	16·5	97·50
„ 14........	132·75	56·0	69·0	14·0	16·0	
„ 15........	131·79	60·0	70·0	14·5	15·5	97·50
Averages	131·67	57·65	69·15	15·05	15·6	97·27

Experiment II, 1882. June 7 to June 15.

The Work done June 11, 1882.

Table H.

Distance walked	32 miles.
Total time of journey..............	8¼ hours.
Halts	1¼ „
Actual time of walking	7 „
Average pace	4·57 miles per hour.
Weight (naked) before starting	131·25 lbs.
„ on returning	126·75 „
Loss	4·50 „
Weight of clothes and kit, *i.e.*, load carried	27·75 „

Remarks.—The walk was accomplished very easily in the time, and without material fatigue. I was perfectly recovered on the following day.

Experiment II, 1882.

	Whole experiment.	Daily.
Nitrogen of urine	131·60	14·62
„ fæces	19·05	2·11
Total excreta	150·65	16·74
„ ingesta	158·78	17·64
Difference	8·13	0·90
P_2O_5 of urine	17·07	1·89
P_2O_5 of fæces	18·27	2·03
Total excreta	35·34	3·92
„ ingesta	34·84	3·87
Difference	0·50	0·05

	Before work.	After work.	Difference.
Nitrogen of urine.	13·77	15·29	1·72
„ fæces.	1·19	2·65	1·46
Total excreta	15·22	17·95	2·73
P_2O_5 of urine....	1·97	1·83	0·14
P_2O_5 of fæces....	1·62	2·35	0·73
Total excreta	3·59	4·19	0·60
H_2SO_4 in urine ..	2·74	2·97	0·23

Experiment III. July 5th to July 17th.

This was in reality two experiments in one, and must be considered in two separate periods, viz., from July 6th to July 12th inclusive, and from July 6th to July 17th inclusive.

1st Period.

The Nitrogen.—Again an unmistakable increase in the nitrogen discharge is to be noticed. The daily average after the work exceeds the average before the work by 1·26 grams; the reserve of nitrogen was reduced from 11·62 grams to 10·41 grams, a loss of 1·21 grams, which was more than made up the next day. The close correspondence with the diminution occurring in Experiments I and II is worthy of note.

The Phosphoric Anhydride.—The increased discharge after the work is very marked, but the total quantity ingested in the time is by no means accounted for, as the following figures show:—

P_2O_5 ingested July 6 to July 8	11·61	grams.
P_2O_5 excreted July 6 to July 8	8·25	„
Difference	3·36	„
P_2O_5 ingested July 9 to July 12	15·48	„
P_2O_5 excreted July 9 to July 12........	14·08	„
Difference	1·40	„

i.e., the work consumed 1·96 grams of the balance accumulated in the three days which preceded it.

The Sulphuric Acid.—A daily increase of 0·25 gram followed the work. Reference to the tables shows that the daily increase was—

In Experiment I	0·24	gram.
In Experiment II	0·23	„
In Part I, Experiment III ..	0·25	„

figures hardly to be explained as the result of accident.

2nd *Period.*

The period "before the work" is in this case only one of comparative rest, because it includes the work done on July 9th.

The Nitrogen.—Despite the fact that July 9th was a day of exercise, and that a considerable increase in the nitrogen discharge then occurred, the daily difference between the nitrogen discharge before the work of July 13th and that of the period after it is no less than 5·68 grams, or a total excess on the average before the work of 22·72 grams, and this great increase notwithstanding comparison of the ingesta and excreta from July 6th to July 16th, both inclusive, shows a balance in favour of the ingesta of 6·30 grams. The figures of Table E indicate, however, that the drain on the store was hardly over when the fast of July 17th began. I regret exceedingly that I did not continue the observations for three or four days before fasting, as it would have afforded an excellent opportunity for observing the retention of nitrogen in the body, which seems to be the natural consequence of a previous excessive discharge. Even if we take the day of fast into our calculations, it leaves only 3·93 grams of nitrogen to be accounted for, which may, I think, be fairly disposed of as a portion of a previously existing reserve.

The Phosphoric Acid.—Over the whole period, July 6th to July 16th inclusive, there is a balance of 1·24 grams not accounted for in the excreta, and even if we include the day of fast, the ingesta and excreta practically exactly balance one another. There is, however, no doubt whatever in this experiment, that the severe labour produced a very marked increase in the phosphoric anhydride excreted, not only on the day of work but for two or three days after.

The sulphuric acid, like the phosphoric acid, was nearly doubled on the day of work, and very markedly increased for the three succeeding days.

Composition of Urine.

Experiment III, 1882. July 5 to July 17.

Table A.

Date.	Quantity.	Sp. gr.	Chlorides as NaCl.	Phosphates as P_2O_5.	Sulphates as H_2SO_4.	Nitrogen.
	c.c.					
July 5....	500	1013	1·14	1·77	1·52	9·10
„ 6....	600	1018	1·38	2·44	2·94	14·22
„ 7....	810	1019	9·05	1·71	2·36	12·36
„ 8....	1240	1023	16·74	1·30	2·44	12·18
„ 9....	1510	1015	12·78	1·08	3·28	12·47
„ 10....	1010	1025	10·43	1·44	3·30	16·35
„ 11....	990	1021	10·66	1·80	2·47	13·29
„ 12....	940	1022	12·23	1·74	2·28	11·77
„ 13....	1380	1028	14·05	2·90	4·37	20·22
„ 14....	900	1022	5·96	2·22	3·42	19·16
„ 15....	1500	1020	13·08	2·10	2·97	17·85
„ 16....	980	1022	10·51	2·14	2·83	15·86
„ 17....	2220	1008	8·78	1·15	0·93	9·46
Totals	14080	..	125·69	21·99	33·63	175·02
Averages ..	1173	1020	10·47	1·83	2·80	14·58

Composition of Fæces.

Experiment III, 1882. July 5 to July 17.

Table B.

Date.	Time of passing.	Quantity.	Phosphates as P_2O_5.	Nitrogen.	
		grams.			
July 5....	..	rejected.	..	..	Fast.
„ 6 ...	..	none.			
„ 7....	1 P.M.	129	1·07	1·60	
„ 8....	1 A.M.	176	1·46	2·19	
„ 9....	12.15 „	40	0·33	0·50	
„ 10....	11.30 „	205	3·34	3·43	
„ 11....	12 P.M.	150	2·44	2·50	
„ 12.. {	12.30 „	88	1·40	1·63	
	8.30 „	54	0·87	1·01	
„ 13....	..	none.	0·00	0·00	
„ 14 ...	..	none.	0·00	0·00	
„ 15....	5 „	214	4·39	4·28	
„ 16....	3 „	176	2·81	3·05	
„ 17....	..	none.	0·00	0·00	Fast.
„ 18....	9 „	150	2·40	2·61	
Totals .	..	1382·5	20·51	22·80	
Averages	..	106·3	1·57	1·75	13 days.
	..	125·6	1·86	2·07	11 days.

Total Daily Elimination of Nitrogen by Urine and Fæces.
Experiment III, 1882. July 5 to July 17.
Table C.

Date.	Nitrogen of urine.	Nitrogen of fæces.	Totals.	
July 5	9·10	0·00	9·10	Fast.
„ 6	14·22	1·60	15·82	
„ 7	12·36	2·19	14·55	
„ 8	12·18	0·50	12·68	
„ 9	12·47	3·43	15·90	
„ 10	16·35	2·50	18·85	
„ 11	13·29	1·63	14·92	
„ 12	11·77	1·01	12·78	
„ 13	20·22	0·00	20·22	
„ 14	19·16	4·28	23·44	
„ 15	17·85	3·05	20·90	
„ 16	15·86	2·61	18·47	
„ 17	9·46	0·00	9·46	Fast.

Daily Balance of Nitrogen of Ingesta and Excreta.
Experiment III, 1882. July 5 to July 17.
Table D.

Date.	Nitrogen of ingesta.	Nitrogen of excreta.	Difference.	
July 5	0·00	9·10	..	Fast.
„ 6	17·64	15·82	1·82	
„ 7	35·28	30·37	4·91	
„ 8	52·92	43·05	9·87	
„ 9	70·57	58·95	11·62	
„ 10	88·21	77·80	10·41	
„ 11	105·85	92·72	13·13	
„ 12	123·50	105·50	18·00	
„ 13	141·14	125·72	15·42	
„ 14	158·78	149·16	9·62	
„ 15	176·42	170·06	6·36	
„ 16	194·06	188·53	5·53	
„ 17	194·06	197·99	−3·93	Fast.

Total Daily Elimination of P_2O_5.

Experiment III, 1882. July 5 to July 17.

Table E.

Date.	P_2O_5 of urine.	P_2O_5 of fæces.	Total.
July 5	1·77		
,, 6	2·40	1·07	3·47
,, 7	1·71	1·46	3·17
,, 8	1·30	0·33	1·63
,, 9	1·08	3·34	4·42
,, 10	1·44	2·44	3·88
,, 11	1·80	1·40	3·20
,, 12	1·74	0·87	2·61
,, 13	2·90	0·00	2·90
,, 14	2·22	0·00	2·22
,, 15	2·10	4·39	6·49
,, 16	2·14	2·81	4·95
,, 17	1·15	2·40	3·55
Totals	21·99	20·51	42·50
Averages	1·99	1·86	3·86

Observations of Pulse, Respiration, Temperature, and Body-Weight.

Experiment III, 1882. July 4 to July 18.

Table F.

Date.	Time.	Pulse.		Respiration.		Body-weight.	Temperature.	
		L.	S.	L.	S.		Mouth.	Axilla.
						lbs.	F°.	F°.
July 4....	10.30 A.M.	53	74	13	15	128·31	..	96·6
,, 5....	2 ,,	50	64	15	18	129·00	..	97·0
,, 5....	9·45 ,,	50	70	15	14	128·00	..	97·0
,, 5....	12 P.M.	51	72	14	18	127·50	..	97·6
,, 6....	9 A.M.	51	64	14	16	126·25	..	97·2
,, 7....	12.30 ,,	68	74	16	17	130·75	..	97·8
,, 7....	9.45 ,,	54	58	14	14	129·50	..	97·2
,, 7....	12 P.M.	60	70	15	16	132·25	..	97·0
,, 8....	9 A.M.	50	50	13	15	130·75	..	96·8
,, 9....	2.30 ,,	57	74	16	18	131·50	..	97·2
,, 9....	8 ,,	56	58	16	16	130·75	..	97·2
,, 9....	3 P.M.	..	..	..	..	..	99·6	
,, 9....	11 ,,	62	72	16	20	..	..	97·8
,, 10....	7.30 A.M.	53	53	12	13	127·25	..	97·8
,, 11....	2 ,,	59	90	14	19	130·25	..	97·8
,, 11....	10.30 ,,	49	64	15	16	128·75	..	97·8
,, 12....	12 15 ,,	55	62	14	15	132·75	..	97·2
,, 12....	7.30 ,,	51	50	13	16	130·00	..	96·6
,, 12....	11.30 P.M.	62	80	15	15	131·25	..	97·4
,, 13....	4.30 A.M.	52	60	13	16	130·75	..	96·0

Table F—*continued.*

Date.	Time.	Pulse.		Respiration.		Body-weight.	Temperature.	
		L.	S.	L.	S.		Mouth.	Axilla.
						lbs.		
July 13..	8.15 A.M.	..	..	..	..	..	99·4	
,, 13..	10 ,,	..	..	..	..	..	98·6	
,, 13..	7.30 P.M.	80	..	..	..	124·00	99·2	
,, 13..	11.45 ,,	64	92	17	18	126·50	..	98·2
,, 14..	9.30 A.M.	49	66	10	11	124·75	..	96·8
,, 15..	3 ,,	62	68	12	16	129·50	..	95·9
,, 15..	11 ,,	50	70	11	12	128·00	..	96·8
,, 16..	3 ,,	56	64	14	14	129·00	..	97·1
,, 16..	11 ,,	54	68	12	13	127·50	..	96·8
,, 17..	2.30 ,,	52	64	13	14	130·00	..	97·4
,, 17..	11 ,,	52	52	13	14	129·00	..	96·8
,, 18..	12.30 ,,	56	100	13	13	126·50	..	97·2
,, 18..	8.30 ,,	48	..	..	..	124·50	..	98·0

Mean Daily Body-Weight, Pulse, and Temperature.

Experiment III, 1882. July 4 to July 17.

Table G.

Date.	Body-weight.	Pulse.		Respiration.		Axillary temperature.
		L.	S.	L.	S.	
	lbs.					F°.
July 4	128·62	51·5	69·0	14·0	16·5	96·6
,, 5	127·75	50·5	71·0	14·5	16·0	97·3
,, 6	128·50	59·5	69·0	15·0	16·5	97·5
,, 7	130·87	57·0	64·0	14·5	15·0	97·1
,, 8	131·12	53·5	62·0	14·5	16·5	97·0
,, 9	126·75	59·0	68·0	16·0	18·0	97·5
,, 10	128·75	56·0	71·5	13·0	16·0	97·8
,, 11	130·75	52·0	63·0	14·5	15·5	97·5
,, 12	130·62	56·5	65·0	14·0	15·5	97·0
,, 13	127·07	58·0	76·0	15·0	17·0	97·1
,, 14	127·12	55·5	67·0	11·0	13·5	96·3
,, 15	128·50	53·0	67·0	12·5	13·0	96·9
,, 16	128·65	53·0	66·0	12·5	13·5	97·1
,, 17	127·75	54·0	76·0	13·0	13·5	97·0
Averages.....	128·77	54·9	68·4	13·9	15·4	97·12

Experiment III, 1882. *July 6th to July 16th.*

Two work days were introduced into the experiment, with the view of testing the effect of muscular labour upon the elimination of nitrogen at a time when it might be supposed that the reserve of nitrogen had been considerably drawn upon by previous exercise, and before there was time to accumulate a fresh store.

Work done, July 9.

Table H.

Distance walked	33 miles.
Total time of journey	9 hours.
Halts	2 ,,
Actual time of walking	7 ,,
Average pace	4·71 miles per hour.
Load carried	27 lbs.

I was prevented from observing my body-weight at the end of the journey, as I did not return to town the same evening, and a weighing machine was not accessible.

The loss of weight may, I think, be reckoned at about 4 lbs., considering the difference between the weight before starting and the weight taken the next day.

Work done, July 13, 1882.

Table H.

Distance walked	47 miles.
Total time of journey	$11\frac{1}{4}$ hours.
Halts	$1\frac{1}{4}$ hour.
Actual time of walking	10 hours.
Average pace	4·7 miles per hour.
Weight (naked) before starting	130·75 lbs.
,, on returning	124·00 ,,
Loss	6·75 ,,
Load carried	27·75 ,,

This journey was one of the greatest efforts I have ever made in the course of these experiments. The first 16 miles was covered in 3 hours 10 minutes, during the greater part of which time it rained heavily, and the roads were consequently in very bad condition. I was excessively tired on my return, but by no means exhausted. Food and rest soon dissipated the effects of the walk, and the next morning I was none the worse for the exertion, with the exception that I was a little stiff, and suffered all day from intense thirst. I may here remark that on every occasion on which I have sustained con-

siderable loss of weight in consequence of severe exercise, I have observed this extreme thirst on the day following. It would seem that a compensation for the weight lost, in the shape of water, was absolutely necessary.

Comparison of Nitrogen and Phosphoric Acid in the Ingesta and Excreta.

Experiment III, 1882.

	12 Days.	Daily.	11 Days.	Daily.
Nitrogen of urine	175·02	14·58	165·56	15·05
,, fæces	22·80	1·90	22·80	2·07
Total excreta	197·82	16·48	188·36	17·12
,, ingesta	194·06	16·17	194·06	17·64
Difference	3·76	0·31	6·30	0·52
P_2O_5 of urine........	21·99	1·83	20·84	1·89
P_2O_5 of fæces........	20·51	1·70	20·51	1·86
Total excreta	42·50	3·54	41·35	3·75
,, ingesta	42·59	3·55	42·59	3·87
Difference	0·09	0·00	1·24	0·12

Comparison of Nitrogen, Phosphoric and Sulphuric Acids Excreted before and after the Work.

Experiment III, 1882.

1st Period.

	Before work.	After work.	Differences.
Nitrogen of urine	12·92	13·47	0·55
,, fæces	1·43	2·14	0·71
Total nitrogen	14·35	15·61	1·26
P_2O_5 of urine..........	1·80	1·51	0·39
P_2O_5 of fæces..........	0·95	2·01	1·06
Total P_2O_5	2·75	3·52	0·77
H_2SO_4 of urine	2·58	2·83	0·25

2nd Period.

Nitrogen of urine	13·23	18·27	5·04
,, fæces	1·83	2·43	0·60
Total nitrogen	15·07	20·75	5·68
P_2O_5 of urine..........	1·63	2·35	0·72
P_2O_5 of fæces..........	1·56	2·40	0·84
Total P_2O_5	3·19	4·75	1·56
H_2SO_4 of urine	2·72	3·40	0·68

Experiment, February 5th to March 3rd, 1883.

This experiment was conducted entirely for the purpose of determining the amount of the nitrogenous reserve, of the existence of which, in one sense or another, I now feel thoroughly assured.

The plan adopted was, after having lived on the experimental diet for some time, to abstain from food once or twice, in order to see whether the amount of nitrogen discharged during a twenty-four hours' fast was much influenced by the amount of food which had been taken for some days previously. I think I may say in starting, that the result was a negative one, that is to say, that the amount discharged in a twenty-four hours' fast seems to be practically a constant under ordinary circumstances.

It further seemed to me probable, or at all events possible, that as now nearly six years have elapsed since the original observations were made which determined my experimental diet, I might be habitually taking more or less food per diem than I did six years ago.

In order to test this, from February 6th to February 13th inclusive I collected my excreta, whilst living upon such a diet as I have been accustomed to during that period, without any regard to experimental considerations. I ate, drank, slept, and worked as though no experiment were in progress, and in order to avoid any possible influence which such observations might have had upon my mode of life, I abstained from noticing the variations of body-weight, temperature, nitrogen discharge, &c., as I have found that when I am obliged to make such observations myself, I cannot avoid the effect of consideration of them upon my daily life. For instance, I feel quite certain that had I known that on any particular day the nitrogen discharge was higher or lower than I might have expected, the knowledge of it would unquestionably have affected the amount of food I should have taken on the following day. This may seem a somewhat extraordinary admission to make, but nevertheless I am convinced of its truth. During the whole of this first period, therefore, I was careful not to inform myself of the results of the analyses. On February 14th and 15th I abstained from all food whatsoever. On February 16th I began the ordinary experimental diet, and continued it until February 26th. On February 27th I again abstained from all food, returning on the 28th to my diet, which was continued until March 3rd. This experiment then naturally divides itself into two parts, the period of ordinary diet and the period of experimental diet, and we have to consider the effect of the fasting in each case.

Inspection of the figures, particularly those which show the daily discharge of nitrogen by the urine, suffices to demonstrate the great similarity which exists between my ordinary food and my experimental diet.

I may incidentally remark that during these six years the variations of my body-weight have been very insignificant, which is, I think, an additional proof that, at all events as far as food is concerned, I have lived, without knowing it, almost according to rule, though it will appear in the sequel that my ordinary diet was in some respects rather more generous than the experimental one.

With this introduction we may proceed to discuss the analytical details of the experiment.

Composition of Urine.

Experiments, February 5 to March 3, 1883.

Table A.

Date.	Quantity.	Chlorides.	Phosphates.	Sulphates.	Nitrogen by combustion.	Nitrogen by hypobromite.	Sp. gr.
Feb. 5	190	9·83	2·47	3·33	14·50	13·08	1014
„ 6	1200	7·58	2·14	3·22	13·01	12·13	1020
„ 7	1400	8·96	2·32	3·71	13·87	12·86	1017
„ 8	2620	14·20	2·60	4·77	18·06	16·19	1014
„ 9	2100	10·14	2·18	3·93	15·05	13·29	1015
„ 10	2000	7·91	2·56	3·59	15·15	14·17	1012
„ 11	1200	5·38	1·62	2·36	9·63	8·81	1013
„ 12	1500	7·94	2·35	4·28	14·27	13·21	1017
„ 13	2200	12·73	2·31	4·12	15·79	14·43	1013
„ 14*a*	800	4·10	0·78	1·15	5·98	5·67	1010
„ 14*b*	800	0·30	0·87	0·65	3·32	3·50	1008
„ 15*a*	500	2·03	0·80	0·84	5·49	5·19	1017
„ 15*b*	900	0·25	1·02	0·64	3·76	3·39	1006
„ 16	1200	2·64	2·37	2·92	14·72	13·70	1017
„ 17	2000	11·90	1·86	2·69	13·99	13·51	1014
„ 18	2300	19·40	1·63	2·95	13·58	12·64	1015
„ 19	1300	13·35	1·57	2·84	12·21	11·71	1024
„ 20	1500	18·22	1·70	2·86	13·90	13·07	1023
„ 21	1700	18·01	1·64	2·44	12·72	12·70	1017
„ 22	2100	20·63	1·99	2·93	13·33	13·42	1017
„ 23	1300	17·49	1·54	2·78	13·43	13·00	1027
„ 24	1800	20·03	1·80	2·90	15·38	16·06	1021
„ 25	1400	11·92	1·40	2·36	11·07	10·89	1017
„ 26	1600	17·15	1·76	2·99	15·05	13·83	1021
„ 27	2000	9·78	1·16	1·12	8·66	7·83	1009
„ 28	1200	8·56	1·92	2·76	18·82	11·60	1020
Mar. 1	2700	11·55	1·62	2·11	11·08	10·27	1008
„ 2	1000	4·12	1·73	2·48	11·58	10·89	1019
„ 3	1800	16·73	1·80	2·81	14·12	13·18	1017

Composition of Fæces.

Experiments, February 7 to March 5, 1883.

Table B.

Date.	Quantity.	Nitrogen.	P_2O_5.
	grams.		
February 7	240·0	3·83	3·23
,, 8	171·0	2·73	2·30
,, 9	83·0	1·32	1·11
,, 10	100·0	1·59	1·34
,, 11	130·0	2·07	1·75
,, 12	104·0	1·66	1·40
,, 13	110·0	1·75	1·48
,, 14	90·0	1·43	1·21
,, 15	none	0·00	0·00
,, 16	81·0	1·29	1·09
,, 17	110·0	1·93	1·37
,, 18	79·5	1·39	0·99
,, 19	141·0	2·48	1·76
,, 20	77·0	1·35	0·96
,, 21	153·0	2·69	1·91
,, 22	136·0	2·39	1·69
,, 23	50·0	0·88	0·62
,, 24	123·5	2·17	1·54
,, 25	93·0	1·63	1·16
,, 26	107·0	1·88	1·33
,, 27	164·5	2·89	2·05
,, 28	none	0·00	0·00
March 1	none	0·00	0·00
,, 2	134·5	2·36	1·67
,, 3	73·0	1·28	0·91
,, 4	144·5	2·54	1·80
,, 5	98·0	1·72	1·22
	2893·5	47·15	35·89

Daily Discharge of Nitrogen by Urine and Fæces.

Table C.

Date.	Nitrogen of urine.	Nitrogen of fæces.	Totals.
February 5	14·50	3·83	18·33
" 6	13·01	2·73	15·74
" 7	13·87	1·32	15·19
" 8	18·06	1·59	19·65
" 9	15·05	2·07	17·12
" 10	15·15	1·66	16·81
" 11	9·63	1·75	11·38
" 12	14·27	1·43	15·70
" 13	15·79	0·00	15·79
" 14	9·30	1·29	10·59
" 15	9·26	..	9·26
	147·89	17·67	165·56
" 16	14·72	1·93	16·65
" 17	13·99	1·39	15·38
" 18	13·58	2·48	16·06
" 19	12·21	1·35	13·56
" 20	13·90	2·69	16·59
" 21	12·72	2·39	15·11
" 22	13·33	0·88	14·21
" 23	13·43	2·17	15·60
" 24	15·38	1·63	17·01
" 25	11·07	1·88	12·95
" 26	15·05	2·89	17·94
" 27	8·66	0·00	8·66
" 28	12·82	2·36	15·18
March 1	11·08	1·28	12·36
" 2	11·58	2·54	14·12
" 3	14·12	1·72	15·84
	207·64	29·58	237·22

Daily Balance of Ingesta and Excreta.

Experiments, February 5 to March 3, 1883.

Table D.

Date.		Nitrogen of excreta.	Nitrogen of ingesta.	Difference.	Nitrogen of excreta.	Nitrogen of ingesta.	Difference.
Feb.	5	18·33	17·64*	− 0·69			
,,	6	34·07	35·28	+ 1·21			
,,	7	49·26	52·92	+ 3·66			
,,	8	68·91	70·57	+ 1·66			
,,	9	86·03	88·21	+ 2·18			
,,	10	102·84	105·85	+ 3·01			
,,	11	114·22	105·85	− 8·37			
,,	12	129·92	123·50	− 6·42			
,,	13	145·71	141·14	− 4·57			
,,	14	156·30	141·14	−15·16			
,,	15	165·56	141·14	−24·42			
,,	16	182·21	158·19	−24·02	16·65	17·05	+ 0·40
,,	17	197·59	172·43	−25·16	32·03	31·29	− 0·74
,,	18	213·65	190·07	−23·58	48·09	48·94	+ 0·85
,,	19	227·21	207·71	−19·50	61·65	66·73	+ 5·08
,,	20	243·80	225·49	−18·31	78·24	84·37	+ 6·13
,,	21	258·90	243·13	−15·77	93·35	102·01	+ 8·66
,,	22	273·11	260·77	−12·34	107·56	119·65	+12·09
,,	23	288·72	278·41	−10·31	123·16	137·30	+14·14
,,	24	305·73	296·05	− 9·68	140·17	154·94	+14·77
,,	25	318·68	313·69	− 4·99	153·12	172·58	+19·46
,,	26	336·62	331·33	− 5·29	171·06	190·23	+19·17
,,	27	345·28	331·33	−13·95	179·72	190·23	+10·51
,,	28	360·46	348·97	−11·49	194·90	207·87	+12·97
Mar.	1	372·82	366·61	− 6·21	207·26	225·51	+18·25
,,	2	386·94	379·64	− 7·30	221·38	238·54	+17·16
,,	3	402·78	397·28	− 5·50	237·22	256·19	+18·97

* In the construction of this table it has been assumed that the diet February 5th to February 15th was the same as the regulated experimental diet.

Balance of P_2O_5.

Experiments, February 5 to March 3, 1883.

Table E.

Date.	P_2O_5 of urine.	P_2O_5 of fæces.	Totals.
February 5	2·47	3·23	5·70
" 6	2·14	2·30	4·44
" 7	2·32	1·11	3·43
" 8	2·60	1·34	3·94
" 9	2·18	1·75	3·93
" 10	2·56	1·40	3·96
" 11	1·62	1·48	3·10
" 12	2·35	1·21	3·56
" 13	2·31	0·00	2·31
" 14	1·65	1·09	2·74
" 15	1·82	—	1·82
Totals	24·02	14·91	38·93
February 16	2·37	1·37	3·74
" 17	1·86	0·99	2·85
" 18	1·63	1·76	3·39
" 19	1·57	0·96	2·53
" 20	1·70	1·91	3·61
" 21	1·64	1·69	3·33
" 22	1·99	0·62	2·61
" 23	1·54	1·54	3·08
" 24	1·80	1·16	2·96
" 25	1·40	1·33	2·73
" 26	1·76	2·05	3·81
" 27	1·16	0·00	1·16
" 28	1·92	1·67	3·59
March 1	1·62	0·91	2·53
" 2	1·73	0·80	3·53
" 3	1·80	1·22	3·02
Totals	27·49	20·88	48·37

Observations of Pulse, Temperature, Respiration, and Body-Weight.

Experiments, February 12 to March 3, 1883.

Table F.

Date.	Time.	Pulse. L.	Pulse. S.	Respiration. L.	Respiration. S.	Body-weight.	Temperature. Axilla.
						lbs.	F.°
February 12	9 A.M.	49	80	12	14	..	97·0
„ 13	9·15 „	50	60	13	14	..	97·2
„ 14	3·15 „	58	64	13	15	..	97·2
„ „	9 „	56	69	12	14	134·00	97·2
„ 15	1·30 „	46	62	13	15	131·75	95·8*
„ „	9 „	52	64	15	15	131·62	96·8
„ 16	1 „	52	68	13	14	131·12	97·0†
„ „	9 „	50	54	13	15	130·00	97·2
„ 17	2·30 „	52	58	13	13	133·25	96·8
„ „	10 „	48	54	12	15	132·50	97·2
„ 18	11 „	48	48	13	14	132·75	97·3
„ 19	4 „	59	80	14	14	133·75	97·4
„ „	11 „	58	..	16	..	132·50	97·2
„ 20	3 „	48	48	13	13	134·50	96·3
„ „	9 „	48	52	13	14	134·00	97·2
„ 21	4 „	51	..	13	15	135·00	96·5
„ „	10 „	52	..	13	..	134·50	97·3
„ 22	3 „	53	64	14	15	136·00	97·4
„ „	9 „	54	..	12	..	134·75	97·4
„ 23	5·30 „	44	..	12	..	134·00	96·5
„ „	10 „	54	..	13	..	132·75	98·0
„ 24	4 „	62	89	15	18	133·25	
„ „	8·45 „	58	..	14	..	134·00	97·6
„ 25	2·30 P.M.	51	..	13	..	132·50	97·7
„ 26							
„ 27	3 A.M.	65	..	12	..	136·50	97·0
„ „	10·30 „	54	..	12	..	134·25	97·6
„ 28	2·30 „	52	102	12	23	130·25	98·0‡
„ „	9·30 „	56	..	12	..	130·25	98·0
March 1							
„ 2	10·30 „	52	..	12	..	130·25	97·2
„ 3	6 „	52	..	12	..	132·75	96·9

* February 15.—Temperature taken five times very carefully.

† February 16.—Pulse very irregular.

‡ February 28.—After running 300 yards.

Mean Body-Weight, Pulse, Respiration, and Temperature.

Experiments, February 13 to March 2, 1883.

Table G.

Date.	Body-weight.	Pulse.		Respiration.		Axillary temperature.
		L.	S.	L.	S.	
	lbs.					F°.
February 13......	..	54	62	13	14·5	97·2
" 14......	132·87	54	65·5	12·5	14·5	96·5
" 15......	131·37	51	66	14	14·5	96·3
" 16......	131·60	52	56	13	14	97·0
" 17......	133·25	51	51			
" 18......	133·25	..	..	13·5	14	97·35
" 19......	133·50	53·5	64	14·5	..	96·70
" 20......	134·50	53	48	13	14·5	96·85
" 21......	135·25	49·5	52	13·5	15	97·35
" 22......	134·37	52·5	64	12	..	96·95
" 23......	133·00	49	..	14		
" 24......	134·16	58				
" 25 & 26.	132·50	..	..	12·5	..	97·35
" 27......	132·25	54	..	12	..	97·80
" 28......	130·25					
March 1 & 2...	131·50	52	..	12	..	97·05
Averages	132·90	52·6	58·7	13	14·4	97·03

Division I. February 5th to February 15th.

The nitrogen of the urine from February 5th to February 10th inclusive, presents very little that is worthy of remark, unless it be that the sixth and seventh were apparently days of small elimination, the ninth and tenth of moderate, and the eighth one of excessive discharge. The daily average from February 5th to February 10th inclusive was 14·94 grams; the averages of the experiments of 1882 were as follows:—

I. 15·03 grams.
II. 14·62 ,,
III. 14·58 ,,

giving an average for the three experiments of 14·74 grams, showing I think that my assumption that the diet during the 1st division of the 1883 experiments was very nearly the same as the special experimental diet, is justifiable.

On February 11th, a day of fast, the discharge by the urine was 9·63 grams, on February 14th 9·30 grams, on February 15th 9·26 grams, on February 27th 8·66 grams; also days of abstinence from food.

It is curious to observe how on these days of fast, despite different circumstances, and the fact that the fast of February 15th was preceded by a fast on the previous day, how great is the similarity in the daily discharge, for again on July 5th, 1882, a day of fast previous to commencing an experiment, the discharge was 9·10 grams, and on July 17th, at the conclusion of the same experiment, 9·46 grams were discharged.

This gives an average discharge on fast days of 9·23 grams of nitrogen by the urine.

Now turning to the table showing the daily balance of nitrogen in the food and excreta, we find in the first part of this experiment that assuming, as I have endeavoured to show we have some right to assume, that the daily ingestion of nitrogen was practically the same as during the experimental diet, it will be seen that I started with a deficit of 0·69 gram, which became a balance of 3·01 grams on February 10th. After considerable fluctuations on the 8th and 9th, the fast of February 11th reduced the balance to a deficit of 8·37 grams, that is to say, 11·38 grams were removed from the body, and it is a very extraordinary fact, though I fear accident has something to do with it, that the total nitrogen discharged on February 11th by urine and fæces amounted to 11·38 grams precisely. Supposing this to be a fact, and not a mere coincidence, the whole of the nitrogen excreted on February 12th should be represented by the difference between the supposed ingesta and this deficit of 11·38 grams, that is to say, 6·26 grams is all that the body can afford; but this is not the case. In this case as in other instances the deficit is not made up at once, but slowly; 1·94 grams were retained on February 12th, and 1·85 grams on February 13th, thus reducing the deficit to 4·58 grams. On February 14th the fasting raised it again to 15·16 grams, and the fast of February 15th to 24·42 grams, and this closes the first period.

Whatever may be the value of calculations based on this hypothesis of similarity of nitrogenous ingestion, it is, I think, only fair to assume that there was a deficit, put it even as low as 10 grams. On February 15th I began the experimental diet, with the exception of so much as reduced the daily ingestion of nitrogen to 17·05 grams, and despite the apparent needs of the body, out of 17·05 grams taken in, 16·65 grams were discharged. Again on February 17th the deficit reached its maximum of 25·16 grams, gradually diminishing up to February 24th, when it amounted to only 9·68 grams, whilst on February 25th the excretion was so small, that the deficit fell to 4·99 grams, to be increased on the following day to 5·29 grams. The fast of February 27th brought it up to 13·95 grams, which was reduced on March 1st to 6·21 grams, rose again to 7·30 grams on the 2nd, and fell to 5·50 grams on the 3rd, the concluding day of the experiment.

Variations from the Experimental Diet, February 16 to March 2.

Experiments, February 16 to March 2, 1883.

Date.	Quantity.	Food.	Nitrogen.	P_2O_5.	H_2SO_4.	NaCl.	
	grams.						
Feb. 16 ..	60	Potato	0·586	0·295	0·126	0·306	Omitted
„ 17 ..	200	Flour	3·403	0·680	0·032	..	Omitted
„ 20 ..	10	Milk	0·147	0·070	0·003	0·403	Added
Mar. 2 ..	20	Julienne	0·354	0·162	0·088	0·057	Omitted
„ 2 ..	30	Meat	4·065	0·567	0·018	0·051	Omitted
„ 2 ..	20	Potato	0·192	0·098	0·022	0·102	Omitted

I may here remark that the total nitrogen value of the food omitted on February 16th and 17th, and on March 2nd, was 8·60 grams; exact particulars are stated in the table already given. So that had the full diet been taken on these days there would have been a balance of 3·1 grams in the body, or rather it would have been possible. During the whole of the latter part of this experiment, that is from February 10th onwards, my sensations were such as I have learned to associate with the repair of waste, and it would have been exceedingly interesting to have continued the experiment yet another ten or twelve days, in order that I might have had the opportunity of investigating the effect of muscular labour, or of starvation upon my body, at this time presumably in a state of nitrogenous equilibrium.

I do not feel justified in again subjecting myself to prolonged starvation, that is to say starvation extending over four or five days; but I think that similar results can be obtained, as far as the nitrogen is concerned, by mere twenty-four hours' abstinence from food, provided that the discharge of nitrogen has been observed for a sufficiently long time previously, and has been so regulated that we can with some degree of certainty assume that there is a deficiency of this readily dischargeable nitrogen in the body. That there would be a considerable discharge of nitrogen there can be no doubt, but the source of it could hardly be the same as in the well- or I might say over-fed body.

It is, I think, a somewhat extraordinary fact that, on the assumption we have made that the nitrogenous value of the diet from February 5th to February 15th was about the same as the experimental diet, with a deficit of 15·16 grams, a further starvation of twenty-four hours should have produced almost identically the same result as a day's starvation, taken haphazard on the day on which I chose to begin an experiment, such as was the case in Experiment III of 1882. I do not wish to invest this fig. 9 with any undue importance; but it certainly appears to represent some definite

condition, and I shall not be satisfied that this is not the case until I have examined the effect of living for several days on a diet whose daily nitrogenous value shall be only 9 grams, the other constituents remaining as far as possible the same. I should expect to find that twenty-four hours' starvation would result in a discharge of a smaller quantity of nitrogen, say 5 grams, which might be found to be tolerably constant under similar conditions. When this had been determined, two or three days of a non-nitrogenous diet would probably suffice to indicate approximately the daily nitrogenous waste of the body from other sources than food. We have no information of any importance as to the minimum of nitrogen required by the body daily in order to maintain it in health, and I feel sure that experiments on the plan indicated would throw considerable light on the subject.

Conclusion.

The results which have been now put on record, whilst they confirm the conclusions of Dr. Parkes, show that the disturbance produced by very severe labour is much more immediate and of much greater intensity than that which Dr. Parkes observed, the explanation obviously being that in his experiments the exertion imposed on the soldiers who were made the subjects was inadequate.

It has been further shown that, just as in Dr. Parkes' experiment on the effect of privation of nitrogenous food, the diminution of the nitrogen stored in the system was followed by retention, *i.e.*, by a state of things in which the intake was greater than the output; so after the disturbance of the nutrition of the body, which is produced by severe labour, the immediate effect of which is obviously to diminish the store of nitrogenous material in the system, there follows a corresponding diminution of discharge, so that the result is the same, namely, that in Dr. Parkes' words, "an insufficient supply at one time must be subsequently compensated," whether the insufficiency be due either to privation or to exercise.

A third result of importance is this: that this storage of nitrogen is the expression of a tendency of the organism to economise its resources, which is much more constantly operative than has hitherto been supposed.

Thus it appears that whenever the subject of experiment was put upon the regulated experimental diet, without the imposition of more exercise than belongs to an ordinary active life, accumulation took place even when the daily supply of nitrogen did not exceed 17·6 grams, an amount which cannot be regarded as more than adequate to the normal requirements of the organism. This tendency to store nitrogen is therefore a normal endowment of the living body, and, if so, the "retention" which follows starvation or exercise must be

regarded as a mere exaggeration of this tendency, and not as a new process which is set up in the living tissue to compensate for a previous disturbance.

In this, as in other respects, it is obvious that the knowledge which has been gained is only partial.

If, with a nitrogenous income of 17·6 grams per diem, accumulation takes place under normal conditions of life, the minimum adequate supply of nitrogen must be below this. We have no means of stating at present what this adequate supply is, but we have reason to believe that it would be found to differ materially in different individuals, and in the same individual in different states of health. If we could ascertain to what point the supply of nitrogen in any individual could be diminished, without drawing upon his store of nitrogenous material, we should have in this result a perfectly reliable chemical criterion of what may be termed nutritive vigour.

No attempt has hitherto been made to carry out such observations on the human subject, nor do any experimental data exist which would justify even a guess at what the results of a properly conducted investigation would be.

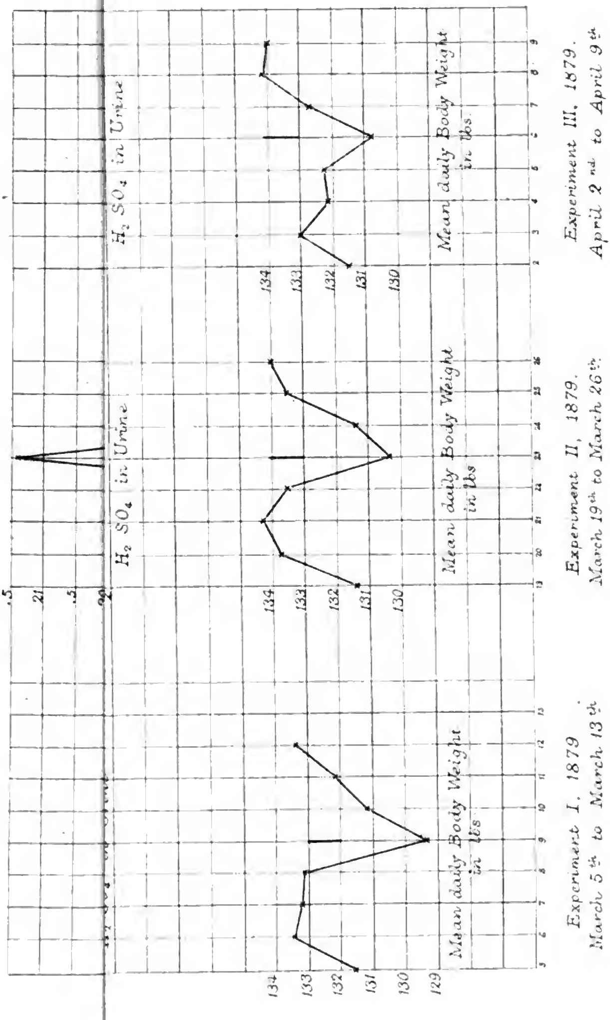
What is first required is to ascertain for how long and in what quantity nitrogen continues to be stored on the diet already mentioned, the period of observation to be at least a fortnight. This having been accomplished, the next step would be to repeat the observation with diets containing less nitrogen than the amount above stated, but otherwise similar to it.

We should then be in a position to inquire how far the increase of nitrogenous output, which has been shown to be the immediate result of labour, is dependent on previous storage, and whether, in conditions of nutrition which involve absence of storage, the effect of severe labour manifests itself in an increased discharge, and finally whether in this case such work can be undergone without producing other disturbances.

A collateral inquiry, which is scarcely less necessary than that which has just been indicated, must as soon as possible be undertaken as to the effect of varying the relation between the time at which food is taken and that at which the work is done.

It has not yet been possible to attempt to enter on this inquiry, which obviously could not be combined with any other. Its importance lies in this, that it would, if successful, afford information as to the question whether or not the organism is capable of at once applying the nitrogenous material of food to meet increased expenditure determined by work.

In the investigations hitherto conducted by me the work done has consisted exclusively of walking. At a future period I hope to substitute other methods of labour, the employment of which will render



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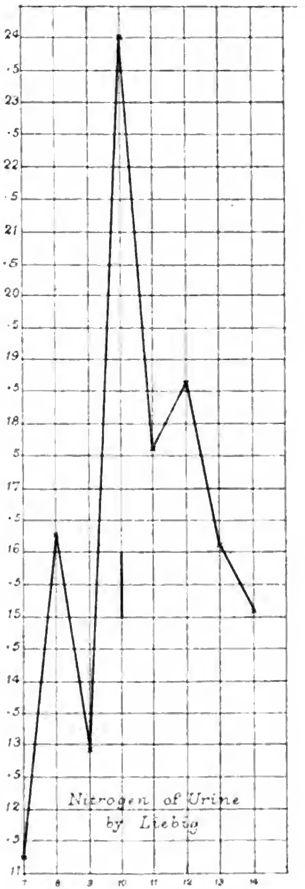

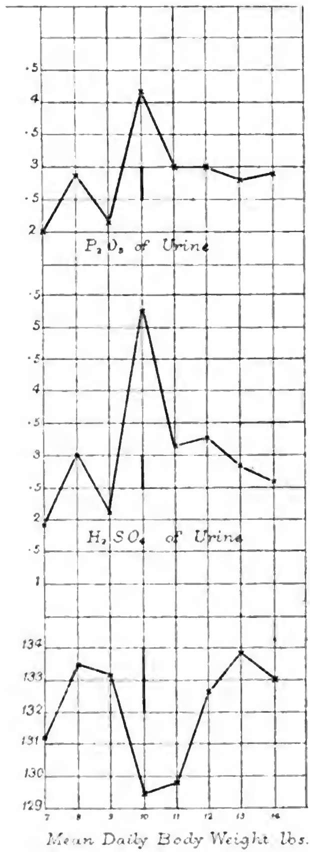

Experiment IV 1879.
May 7th to May 14th

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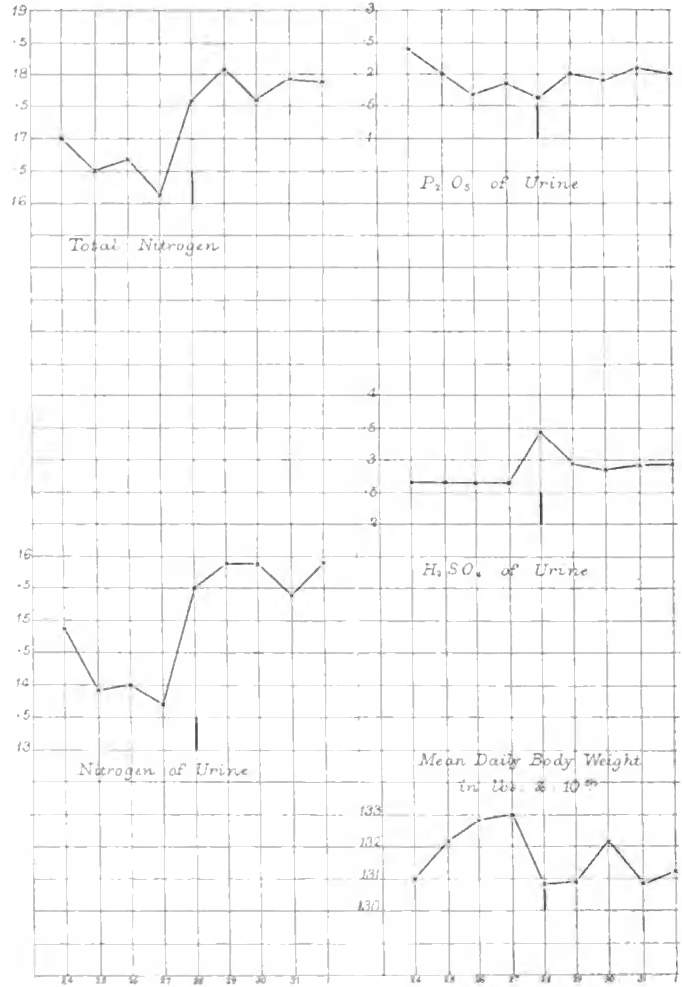

Experiment I. 1882.
May 24th to June 1st.
Vertical line indicates day of work.

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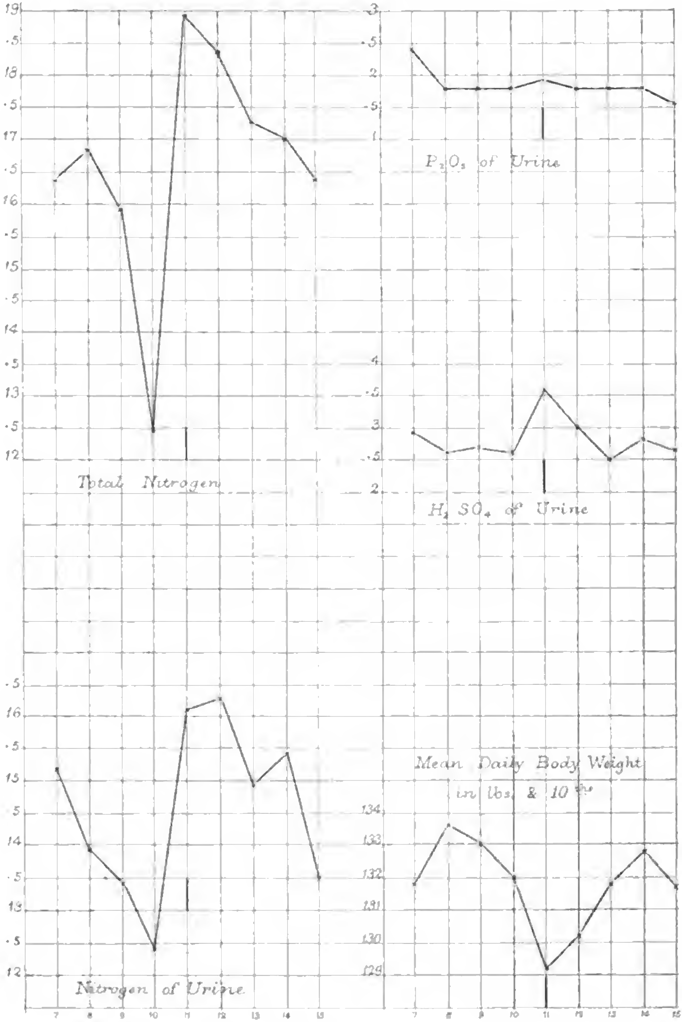

Experiment II 1882.
June 7th to June 15th
Vertical line indicates day of work

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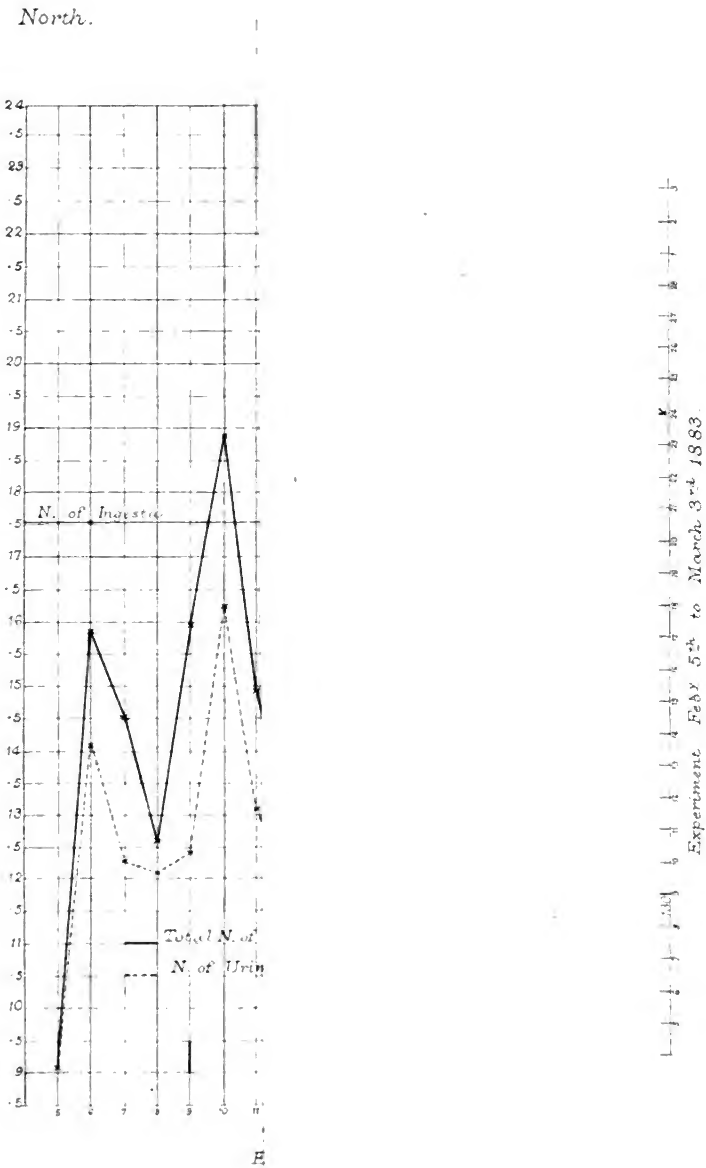
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Experiment Feby 5th to March 3rd 1883

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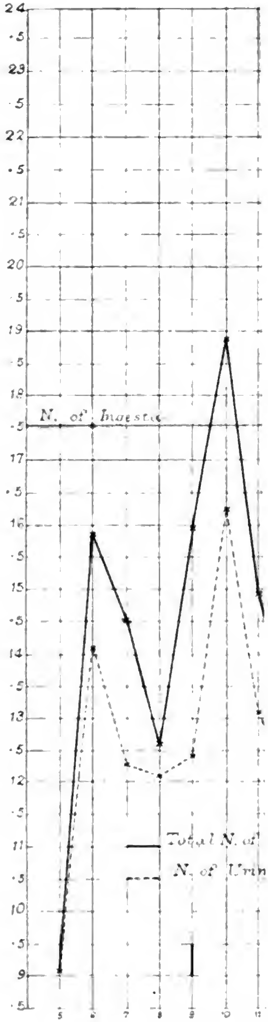

F.

Experiment Feby 5th to March 3rd 1883.

North

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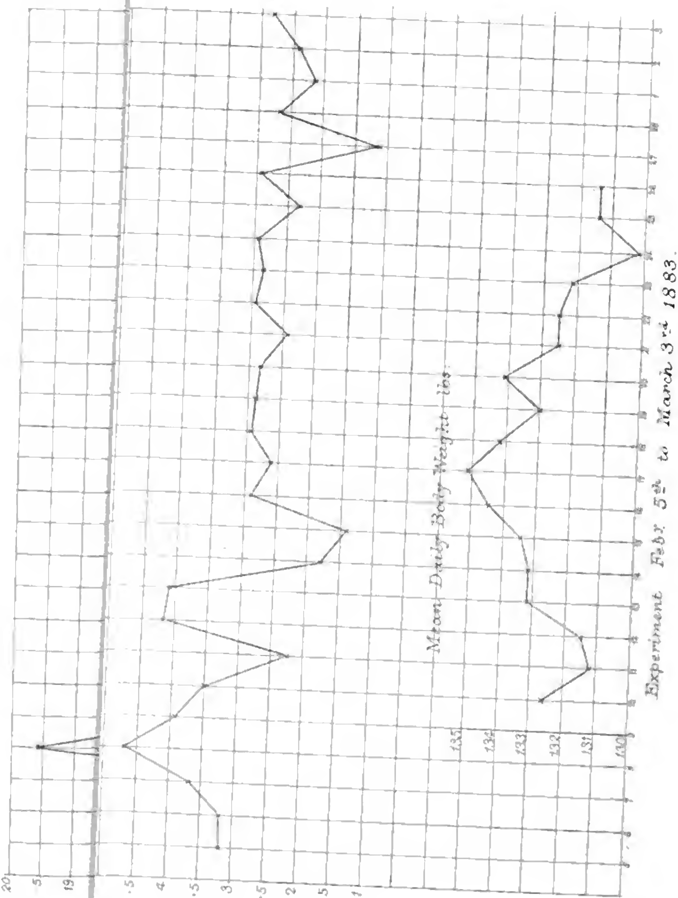


West, Newman & Co. lith.

it possible to vary, as may be expedient, not merely the amount of work done per diem, but the distribution of the work in time. In the realisation of this purpose I anticipate great advantage from the use of the work machine, which I have been able to construct for the purpose of my inquiry by the liberality of the British Association.

In conclusion, I would draw attention to the results relating to the influence of work on the discharge of phosphates and sulphates by the urine.

As regards phosphates, it has been shown that no increase occurs unless the exertion is very severe. As regards sulphates, it manifests itself distinctly in many cases, the output of sulphates being in general terms proportional to that of nitrogenous material.

It is a matter of regret that the total sulphur of the food was not estimated. It is known that the percentage of sulphates contained in the food was insignificant as compared with that excreted in the urine, and consequently almost all of the discharge must have been a product of oxidation.

I beg, in conclusion, to state that the expenses of the present research, which have been extremely heavy, have been defrayed by a grant of the British Medical Association. I desire to express to the Association my most grateful thanks.

"The Influence of Stress and Strain on the Physical Properties of Matter. Part II. Electrical Conductivity (*continued*). The Alteration of the Electrical Conductivity of Cobalt, Magnesium, Steel, and Platinum-iridium by Longitudinal Traction." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received October 7, 1884. Read November 20.

The Alteration of the Electrical Resistance of Cobalt produced by Longitudinal Traction.

In a previous communication to the Royal Society,* I pointed out that whilst with iron the electrical resistance is temporarily increased by temporary longitudinal traction, that of nickel is decreased, provided the stress be not carried beyond a certain limit, and this, too, in spite of the change of dimensions, namely, increase of length and diminution of diameter, which follow from the stress. I further showed† that there is a marked resemblance between the table of "rotational coefficients" drawn up by Professor Hall and that laid down by myself from the results of experiments on the effect of

* "Phil. Trans.," vol. 174, p. 58.

† *Loc. cit.*, p. 168.

mechanical stress on the specific electrical resistance of metals. A comparison of the two tables shows, that with the exception of platinum the metals stand in nearly the same order in both, and that iron and nickel are very conspicuous, the former at the top and the latter at the bottom of both lists.

Mr. Shelford Bidwell has also brought forward* very strong evidence in favour of his assertion, that the "Hall effect" can be explained by the joint action of mechanical strain and certain "Peltier effects," and has shown that those metals in which the "Hall effect" is positive are rendered by temporary traction thermo-electrically negative to pieces of the same metal unstretched. It seemed, then, a matter of considerable interest to ascertain whether cobalt would act like iron or nickel as far as the effect of stress on the electrical resistance is concerned, and I endeavoured—for some time in vain—to obtain either wires or strips of cobalt suitable for the purpose in view. At length, through the courtesy of Mr. Wiggin, jun., Birmingham, I found myself in possession of two strips† of cobalt, upon which I was able to make the necessary experiments.

Preliminary Determination of the Value of "Young's Modulus," Density, &c.

The length of each strip was 58·4 cm., and the thickness and width of one of them, when gauged at ten places at equal distances apart, were as follows:—

Number of observation.	Thickness in centimetres.	Width in centimetres.	Section in square centimetres.
1	0·0864	0·7091	0·06127
2	0·0870	0·7076	0·06156
3	0·0851	0·7131	0·06069
4	0·0858	0·7104	0·06095
5	0·0857	0·7282	0·06241
6	0·0848	0·7250	0·06148
7	0·0850	0·7326	0·06227
8	0·0849	0·7557	0·06416
9	0·0846	0·7556	0·06392
10	0·0854	0·7474	0·06383
Mean	0·08547	0·72847	0·06226

The strip was therefore fairly uniform in section throughout, the mean value of the section as determined by the gauge being 0·06226 square centimetre.

* "Phil. Mag.," April 1884, p. 249.

† Mr. Wiggin informed me that he found it impossible to draw *wires* of cobalt, as the metal was so hard that it destroyed the tools with which it was brought into contact. Messrs. Johnson and Matthey were also good enough to attempt to draw for me wires of cobalt, but they too failed through a like cause.

The density of the metal was determined by means of a specific gravity flask, some pieces having been previously broken off for the purpose, and was found to be 8·231 at a temperature of 16° C. The same pieces were well annealed, and the density at 16° C. was then found to be 8·259. The mean section as determined from the mass of the strip 29·65 grams, the length, and the density, was for the unannealed metal 0·06168 square centimetre, and this value, agreeing as it did fairly with that got by gauging, was assumed to be correct.

The modulus of longitudinal elasticity was determined by holding the strip in the centre and rubbing it along its length with a resined glove. The note obtained was very high in pitch, but the results of the measurements of the number of vibrations obtained by the use of the syren agreed very well with each other.*

Experiment I.

The lower double octave obtained by rubbing the strip longitudinally was taken on a monochord; the syren was then raised to the pitch of the monochord, and the number of vibrations counted for two minutes at a time.

Number of trial.	Number of vibrations recorded by the syren in two minutes.
1	6286 × 20
2	6273 . . .
3	6273 . . .
Mean	6277 × 20

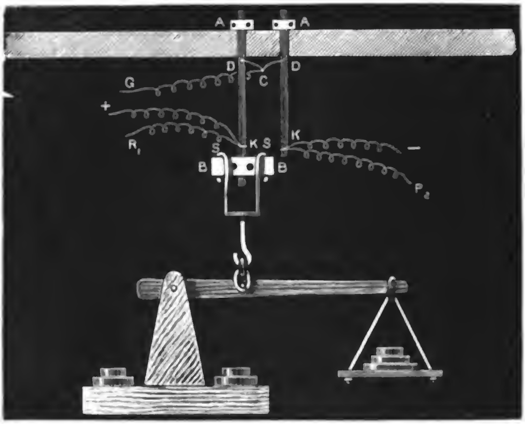
In this experiment the monochord was retuned twice, and it will be observed that the mean value does not differ from any of those forming it by so much as 0·15 per cent. From these last observations the value of "Young's modulus" for the unannealed cobalt was ascertained to be 2005 × 10^6 grams per square centimetre. The same strip was then well annealed, and three results, equally as concordant as the last, were obtained when the strip was thrown into longitudinal vibrations. The value of "Young's modulus" of the metal in the annealed condition was 1817 × 10^6 grams per square centimetre.

Arrangement of the Cobalt Strips for Observation on the Alteration of Resistance produced by Longitudinal Traction.

The two strips (see fig. 1) in the first instance in the unannealed con-

* I have again to thank Mr. Furse, of King's College, for his assistance here.

Fig 1.



dition, passing through two apertures in a table, were secured at their upper extremities to two clamps, A, and the lower extremity of the one to be stretched was fastened to the clamp B, over which passed the double hook S, connected with a stout lever of hard wood. A scale-pan weighing 2 kilos. was suspended at the end of the lever, and by loading and unloading this very carefnlly the strip was subjected to any required alteration of stress. Short pieces of insulated copper wire were soldered to each strip at D, and the junction, C, of these pieces was united with one terminal of the galvanometer. Two other pairs of similar wires were soldered at K, and one of each pair was connected as usual with resistance coils, and the other with one pole of a single Leclanché cell. The two sets of resistance coils, which in this case were about 10 ohms each, were united by a platinum-iridium wire traversed by a sliding-piece connected with the other terminal of the galvanometer. The mode of experimenting was precisely the same, and the same precautions were taken as in the earlier experiments.*

When the strip had been sufficiently tested in the unannealed condition it was well annealed, and in this new condition again experimented on; but whereas before the stress used was not sufficient to cause any permanent elongation, the strip was now permanently lengthened and the permanent alteration of resistance thus caused was measured as well as the temporary alteration. The following experiment was made on the annealed metal when stressed for the first time after the annealing.

* *Loc. cit.*, p. 45 and p. 54.

Experiment II.

Load = W.	Position of sliding-piece.	Alteration of resistance in terms of divisions of the platino-iridium wire. − signifies decrease of resistance on loading.	Temporary alteration of resistance = T.	Total permanent alteration of resistance = P.	$\frac{T}{W}$.	$\frac{P}{W}$.
0	103·0					
2	99·0	− 4·0	− 4·0	0	−2·00	
4	95·0	− 8·0	− 8·0	0	−2·00	
6	93·0	−10·0	−12·0	+ 2·0	−2·00	+0·33
8	93·0	−10·0				
10	93·0	−10·0				
12	98·5	− 4·5	−21·5	+ 17·0	−1·90	+1·42
0	120·0					
2	118·0	− 2·0	− 2·0	+ 17·0	−1·00	
4	115·5	− 4·5	− 4·5	+ 17·0	−1·13	
6	113·5	− 6·5				
8	112·5	− 7·5				
10	111·5	− 8·5				
12	112·0	− 8·0				
14	116·0	− 4·0	−20·0	+ 33·0	−1·43	+2·36
0	136·0					
4	133·0	− 3·0	− 3·0	+ 33·0	−0·75	
8	130·0	− 6·0	− 6·0	+ 33·0	−0·75	
12	128·5	− 8·5				
16	136·0	0	−23·0	+ 56·0	−1·44	+3·50
0	159·0					
4	156·0	− 3·0	− 3·0	+ 56·0	−0·75	
8	153·0	− 6·0	− 6·0	+ 56·0	−0·75	
12	152·0	− 7·0				
16	154·0	− 5·0				
18	160·0	+ 1·0	−23·5	+ 80·5	−1·32	+4·47
0	183·5					
4	181·5	− 2·0	− 2·0	+ 80·5	−0·50	
8	178·5	− 5·0	− 5·0	+ 80·5	−0·63	
12	178·0	− 5·5				
16	179·5	− 4·0				
20	190·0	+ 6·5	−25·5	+112·5	−1·28	+5·63
0	215·5					
4	213·5	− 2·0	− 2·0	+112·5	−0·50	
8	210·5	− 5·0	− 5·0	+112·5	−0·63	
12	209·5	− 6·0				
16	210·5	− 5·0				
20	215·0	− 0·5				
22	223·0	+ 7·5	−28·0	+148·0	−1·27	+6·73
0	251·0					
*0	66·0					
4	68·5	− 2·5	− 2·5	+148·0	−0·63	
8	71·0	− 5·0	− 5·0	+148·0	−0·63	
12	72·0	− 6·0				
16	71·5	− 5·5				
20	69·0	− 3·0				
22	65·5	+ 0·5	−24·5	+173·0	−1·11	+7·86
0	41·0					

* Resistance coils readjusted.

In this experiment the numbers given for the load represent the number of kilograms on the end of the lever,* and, when multiplied by 4908, will represent the actual stress on the strip. After a rest of two days with all the stress off, except that produced by the weight of the lever itself,† the resistance had decreased, so that the sliding-piece had to be shifted from 41 divisions to the right of the zero-point to 45 to the left, or through 86 divisions; so that the total permanent increase of resistance was now represented by 87 divisions instead of 173 divisions. The strip was now tested again with the following results:—

Experiment III.

Load. = W.	Position of sliding-piece.	Alteration of resistance in terms of the divisions of the platino-iridium wire. − signifies decrease of resistance on loading.	Temporary alteration of resistance = T.	Total permanent alteration of resistance = P.	$\frac{T}{W}$.
0	45 left	..	..	+87	
4	48 "	− 3·0	− 3·0	+87	−0·75
8	51 "	− 6·0	− 6·0	+87	−0·75
12	51 "	− 6·0			
16	50 "	− 5·0			
20	48 "	− 3·0	−22·0	+136	−1·10
0	26 "				
20	34 "	− 8·0	−21·0	+119	−1·05
0	13 "				
20	21 "	− 8·0	−21·0	+132	−1·05
0	0 "				
20	10 "	−10·0	−20·0	+142	−1·00
0	10 right				
20	2 left	−12·0	−20·0	+150	−1·00
0	18 right				
20	8 "	−10·0	−19·0	+159	−0·95
0	27 "				
20	16 "	−11·0	−19·0	+167	−0·95
0	35 "				
20	..	..	..	+175	
0	43 "				

* Including the weight of the scale-pan itself.

† This was nearly equal to a load of 1 kilo. on the end of the lever.

20 kilos. were now put on the lever and taken off again six times in succession.

Load =W.	Position of sliding-piece.	Alteration of resistance in terms of the divisions of the platino-iridium wire. − signifies decrease of resistance on loading.	Temporary alteration of resistance =T.	Permanent alteration of resistance =P.	$\frac{T}{W}$.
0	77·0 right	..	..	+209·0	
16	64·0 ,,	−13·0	−14·0	+210·0	−0·875
0	78·0 ,,				
12	68·0 ,,	−10·0	−11·0	+211·0	−0·917
0	79·0 ,,				
8	72·0 ,,	− 7·0	− 6·5	+210·5	−0·813
0	78·5 ,,				
4	74·5 ,,	− 4·0	− 4·5	+211·0	−1·125
0	79·0 ,,				
4	75·0 ,,	− 4·0	− 4·0	+211·0	−1·000
0	79·0 ,,				
4	76·0 ,,	− 3·0	− 4·0	+212·0	−1·000
0	80·0 ,,				
4	75·5 ,,	− 4·5	− 3·5	+211·0	−0·875
0	79·0 ,,				
4	75·0 ,,	− 4·0	− 3·5	+210·5	−0·875
0	78·5 ,,				
8	72·0 ,,	− 6·5	− 7·0	+211·0	−0·875
0	79·0 ,,				
8	72·5 ,,	− 6·5	− 7·0	+211·5	−0·875
0	79·5 ,,				
12	70·0 ,,	− 9·5	−10·5	+212·5	−0·875
0	80·5 ,,				
12	70·5 ,,	−10·0	−10·5	+213·0	−0·875
0	81·0 ,,				
16	69·0 ,,	−12·0	−14·0	+215·0	−0·875
0	83·0 ,,				
20	71·0 ,,	−12·0	−17·5	+220·5	−0·875
0	88·5 ,,				

Remarks on Experiments II and III.

From the third column of Experiment II may be gathered that the resistance of the cobalt *decreases* up to a certain degree of loading, and then begins to increase. The maximum decrease becomes less and less, at first rapidly, and then more slowly as the permanent strain due to the loading becomes greater and greater until a load of 18 kilos.* had been employed. The point of loading at which the maximum decrease occurs becomes higher and higher, gradually increasing from 8 kilos. to about 13 kilos.

* This does not include the stress due to the weight of the lever.

The sixth column shows that the *temporary* alteration of resistance is of the nature of a *decrease*, but the decrease becomes less and less as the permanent strain increases, at first rapidly, and afterwards more slowly, until the load of 18 kilos. has produced its permanent effect. Moreover, we learn from this column that when a load of 12 kilos. had been employed the temporary effect per kilogram is always greater for the highest load than for the smaller ones, so that for the loads 16, 18, and 20 kilos., *when employed for the first time*, we get about twice the temporary decrease per kilogram which is obtained with the loads 2 and 4 kilos. Experiment III, however, teaches us that ultimately, when all the loads have been applied a great number of times, *the temporary decrease is exactly proportional to the load* in the case of the loads 4, 8, 12, 16, and 20 kilos., nor is there any sign, as was the case with nickel, that the decrease of resistance would ultimately be changed to increase when the stress was increased beyond a certain limit. The greatest total stress on the strip, including that caused by the weight of the lever itself, was 1860 kilos. per square centimetre, whereas with nickel the point of loading where the above-mentioned limit was reached, was about 1500 kilos. per square centimetre.* It would seem probable that there is a limiting stress beyond which, as in nickel, the resistance begins to increase with the load, but it is evident from what has been said that this limiting stress is much higher for cobalt than nickel.

From the last values recorded in the sixth column of Experiment III it was calculated that the *decrease* of resistance per unit produced by a stress of 1 gram per square centimetre at the temperature of 16° C. is $386{\cdot}8 \times 10^{-12}$. The ratio of the decrease of resistance per unit to the increase of length per unit† is 0·703, and, if we assume the ratio of lateral contraction to linear elongation to be 0·250, the ratio of the decrease of *specific* resistance per unit to the increase of length per unit of the stress is 2·203. All the values just given are much less than the corresponding ones in the case of nickel.

With the cobalt in the unannealed state temporary traction also produced *decrease* of resistance, but, as might be expected from what has been said above with reference to the effect of permanent strain, the amount of decrease per unit was less than with the annealed metal.‡ The decrease of resistance per unit produced by a stress of

* *Loc. cit.*, p. 60, where the stresses given should be increased by that caused by the weight of the scale-pan if the total stress on the wire is, as in this case, required.

† $\frac{\Delta R}{R} : \frac{\Delta l}{l}$, where R is the resistance and ΔR is the decrease of resistance which results when the length, l, is increased to $l + \Delta l$.

‡ Notice the similarity in this respect between cobalt and nickel. *Loc. cit.*, p. 61.

1 gram per square centimetre was for the unannealed cobalt $242 \cdot 3 \times 10^{-12}$. The ratio of the decrease of resistance per unit to the increase of length per unit is 0·486, and the corresponding ratio in the case of *specific* resistance is 1·986.

Not only is cobalt remarkable for having, like nickel, its resistance decreased by longitudinal traction, in spite of the increase of length and diminution of section which is caused by the stress, but it also presents a peculiarity not seen in nickel or indeed in any of the other metals hitherto examined, namely, the *extreme* persistence with which the same load when applied again and again continues to produce permanent increase of resistance. With few metals is this persistence anything like so noticeable with such stresses per square centimetre as have been here employed, for we may observe from the last two experiments that though a load of 22 kilos. had been twice used on the lever, and afterwards a long rest given to the strip, 20 kilos., after having been put on and taken off fifteen times, still continued to produce permanent increase of resistance.* Even 16 kilos. continued to produce a permanent effect, so that it would be only after a very large number of loadings and unloadings that the increase of resistance on taking off the load would be equal to the decrease of resistance on putting on the load. The permanent increase of resistance caused by the above-mentioned loads also was greater the longer the time during which the stress was maintained.†

The Effect of Permanent Longitudinal Extension on the Specific Resistance of Cobalt.

We have seen that cobalt behaves like nickel as far as the effect on the specific resistance of temporary longitudinal stress is concerned, and that both iron and nickel‡ are decreased in specific resistance by moderate longitudinal strain. This it appears is the case with cobalt also. The distance between the points where the copper wires at the upper and lower extremities of the cobalt strip were soldered, was before stretching 49·8 cm. and after the stretching 50 cm. The specific resistance before the stretching was 2289×10^{-8}, and after the stretching 2231×10^{-8}, so that there was a *decrease* of specific resistance of 2·6 per cent. for a permanent lengthening of 1·8 per cent. The permanent decrease of specific resistance per unit divided by the permanent increase of length per unit, is for iron 0·02, for cobalt 1·44, and for nickel 2·37; so that the permanent

* This was afterwards found to be the case when the same load had been put on and taken off some fifteen times more.

† This "running down" is also explained with other metals, but not to the same extent, as regards persistency with such comparatively small stresses per square centimetre.

‡ *Loc. cit.*, p. 100.

decrease of specific resistance as well as the temporary decrease is greater with nickel than with cobalt.

The Effect of Longitudinal Traction and of Longitudinal Magnetisation on the Thermo-electric Properties of Cobalt.

It will be seen that contrary to the expectation of myself, who had regard to the relationship which apparently exists between the "rotational coefficient" of Hall and the alteration of specific resistance caused by traction, cobalt behaves like nickel and not like iron. I now, therefore, turned to examine the effect of traction on the thermo-electric properties of the metal, for the purpose of ascertaining whether the strip would act in respect to these properties like iron or nickel. According to Bidwell* cobalt acts like iron, but it appears that this experimenter did not subject his bar of cobalt to traction but to *torsion*, and finding that copper under torsion behaved similarly to copper under longitudinal traction, assumed that cobalt would do so likewise. The above-mentioned assumption is hardly, I think, justifiable, and the following experiments show that cobalt is altered by traction in a manner similar to nickel, *provided the metals are not at the same time under the influence of any magnetising stress.*

Experiment IV.

The two strips of cobalt in the unannealed condition were clamped together at their centres, the clamp projecting to a distance of about 3 inches from and at right angles to the strips. One of the strips could be stretched as before by means of the lever, and insulated copper wire soldered near the lower extremities of the two strips served to connect them with the galvanometer. The clamp was then heated at the extremity furthest away from the strips, and the heat conducted along the clamp to the strips was such that in a short time a temperature, as judged roughly by the touch of about 60° C., was attained, stress of moderate amount was then put upon the lever, and this caused a current from stretched to unstretched through the heated junction: consequently *stretched cobalt is thermo-electrically positive to unstretched cobalt.* As soon as the needle of the galvanometer was fairly steady, the stress was removed and a deflection in the opposite direction ensued. The temporary stretching and unstretching were repeated several times, but always with the same result as regards the direction of the deflection. The unannealed cobalt therefore under mechanical stress behaves thermo-electrically like nickel.†

* "Phil. Mag.," April 1884, p. 261.

† In this experiment the strips were under the influence of the earth's vertical magnetic stress, but this last is so much smaller than that due to the helix, that I have not deemed it necessary to take it into account.

Experiment V.

The strips were now dismounted and the same one as had been used before was placed in the axis of a magnetising helix,* especially designed to prevent the heat of the helix from reaching the metal to be magnetised. The two ends of the strip, which projected about 8 inches from either end of the helix, were connected with the galvanometer, and the clamp before used was now at one end of the helix and just outside. The temperature was raised to about 60° C., and as soon as the galvanometer needle had become fairly steady the helix was excited by a single Leclanché cell.† A deflection of the galvanometer needle at once indicated a current from unmagnetised cobalt to magnetised cobalt through the hot junction, and proved that *longitudinally magnetised cobalt is negative to unmagnetised cobalt.* The unannealed cobalt, therefore, under magnetic stress, behaves thermo-electrically in a manner similar to iron. The helix in this experiment was too far from the galvanometer to affect the latter directly, and a reversal of the magnetising current produced a thermo-electric current in the same direction as before. The magnetising force was in this case equal to $4\pi \times 90{\cdot}6 \times 0{\cdot}478 \times C$ in absolute units, where C expresses the magnetising current in absolute units. The electromotive force of the cell was 1·5 volts very nearly, and the resistance in circuit 1·8 ohms, consequently the value of C would be $\frac{1{\cdot}5 \times 10^8}{1{\cdot}8 \times 10^9}$ absolute units, and the magnetising force 43·9 absolute units.

This last result has an important bearing on the question whether Bidwell's explanation of Hall's phenomenon is correct, because, though unmagnetised cobalt is certainly rendered thermo-electrically positive by longitudinal mechanical stress, the case may be different when the metal is magnetised, as in Hall's experiment.‡

Nor indeed would it be safe to assume, without further experiment, that mechanical stress will produce the same effect either in nature or amount on the electrical resistance§ or on the thermo-electric properties of magnetised iron, nickel, and cobalt, as it does on the same metals when not under the influence of magnetic stress.

Experiment VI.

The strip of cobalt used in Experiments IV and V having been annealed, was tested as before for the effect of mechanical stress on

* For a description of this helix, designated as "the coil B," see *loc. cit.*, p. 136.

† One of the more recent kind, and which gives a fairly constant current.

‡ See Sir W. Thomson's paper on "The Effects of Stress on the Magnetisation of Iron, Nickel, and Cobalt," "Phil. Trans.," vol. 170, 1879.

§ I may not, therefore, be right in my conjecture (see "Note on Hall's Phenomenon," "Phil. Mag.," May 1884, p. 402) that in all probability the "Hall effect" on nickel will be diminished by raising the temperature to 100° C.

the thermo-electric properties of the metal. The heating was, however, in this case accomplished by means of an air-chamber, consisting of two concentric brass cylinders with a layer of water between them. The strips were placed, clamped together at their centres, in the axis of the chamber, the two extremities of each projecting about 8 inches from either end of the chamber; a thermometer was also placed with its bulb in the centre of the latter, and the temperature in the first instance raised to 100° C. When the needle of the galvanometer was at rest, a load of 8 kilos. was put on the lever, and after a space of 2 minutes had elapsed a deflection of three divisions of the scale resulted. This deflection again showed the *stressed metal to be positive to the unstretched*, and on the removal of the load the needle came back again to nearly its old position. Trials were made with several loads from 2 to 8 kilos., and always with the same result as regards the nature of the effect; the mean value of the deflection per kilo. being as nearly as could be judged 0·375 division of the scale. The electromotive force developed by the highest load was found to be 0·429 microvolt, and the electromotive force which would be produced by a stress of 1 gram per square centimetre would be 659×10^{-9} microvolts, the temperature of one junction being at 100° C. and of the other about 16° C. The chamber was next permitted to cool down, first to 60° C., and afterwards to 42° C., and at both these temperatures the effect of the stress was found to be in the same direction as before, and though no attempt was made to measure the actual deflection, this was evidently diminished with the temperature.

Finally, it should be added that both in this experiment and in the two previous ones, the extremities of the strips and the clamps at the ends of the stressed strip were all well shielded from the source of heat. It would seem then that we have good reason for concluding that for any temperature between 16° C. and 100° C., cobalt, whether in the annealed or unannealed condition, is rendered by traction thermo-electrically positive to unstretched cobalt.*

The Effect of Excessive Loading on the Electrical Resistance of Hard Piano-steel.

Experiment VII.

I have already shown† that for moderate amounts of stress the effect on the electrical resistance of piano-steel is of the same nature as the effect on iron, but, as according to Mr. H. Johnson,‡ the

* As I thought that want of purity might influence the result, Mr. J. M. Thomson of King's College kindly made an analysis of two small pieces of the metal, and informed me that they contained more than 98 per cent. of cobalt, no trace of nickel, and barely a trace of iron.

† *Loc. cit.*

‡ "The Electrician," May 17th, 1884.

specific resistance of hard piano-steel wire is temporarily diminished by *very considerable* longitudinal traction, I arranged a pair of hard piano-steel wires in the same manner as the nickel wires, of which already mention has been made, and proceeded to test one of them in the usual manner, expecting that whilst moderate stress would increase the specific resistance, excessive stress would diminish it. The loading was accomplished by means of the lever before used, and the results are given below :—

Permanent load on the lever in kilos.	Temporary load in kilos.	Increase of resistance produced by loading in divisions of the platino-iridium wire.	Increase of resistance per kilo. on the lever.
1*	4	160	40·00
1	6	242	40·33
7	6	240	40·00
13	6	242	40·33
		Mean	40·17

It will be seen that the *increase* of resistance produced by the loading is proportional to the load throughout. The diameter of the wire was 0·08246 cm., and the greatest total load on the wire was equal to 102 kilos., or 2 cwts. The greatest stress per square centimetre was above 19,000 kilos., and only a little short of the breaking stress. From the above results was deduced that the increase of resistance per unit produced by a stress of 1 gram per square centimetre, was 1620×10^{-12}, and this number so nearly agrees with those obtained for the previously used specimens of piano-steel, that it was not considered necessary to determine the value of "Young's modulus" and of the simple rigidity, in order to prove that, as in soft iron, the *specific* resistance is *increased*, not only when moderate stresses are employed *but also for stresses close to the breaking load.*† This last experiment, however, confirms the fact mentioned in an earlier paper,‡ that the electrical resistance of steel is less increased than that of iron, not only per gram stress on the square centimetre, but also per unit increase of length.

* This is the load due to the weight of the lever itself.

† I afterwards found that Mr. Johnson must have miscalculated the amount of *temporary lengthening* produced by the load used by him, as he gives it as 3 per cent., though the wire was not loaded beyond the limits of elasticity. This last is an *impossible* result, as the greatest tensile strength of any piano-wire does not admit of a temporary elongation of more than 1·6 per cent. without breaking.

‡ "Proc. Roy. Soc.," vol. 26, p. 401.

The Effect of Longitudinal Traction on the Electrical Resistance of Magnesium.

Cobalt and nickel have been found to decrease in resistance when subjected to moderate longitudinal traction, nor does it appear* that this abnormal behaviour is in any way connected with the magnetic properties of these metals. Nevertheless, it seemed desirable to find, if possible, some metal which, possessing much feebler magnetic properties, would exhibit similar conduct. Now, judging from the apparent relationship between the alteration of specific resistance produced by traction and the "Hall effect," that magnesium might be found to be such a metal, I obtained from Messrs. Matthey and Johnson about 60 feet of magnesium wire, and examined it with the above-mentioned object in view.

Preliminary Determinations of the Value of "Young's Modulus," Simple Rigidity, Density, &c.

"Young's modulus" was, in the first instance, determined by the method of longitudinal vibrations, a length of 550 cm. being under examination. The wire was stretched on a long wooden box, and precautions taken which will be fully described in a future communication to the Society, to avoid certain sources of error incidental to this method. The results obtained with the syren were quite as accordant with each other as those got with the cobalt strip, and from these and the value of the density obtained in the manner presently to be described, was deduced a value of "Young's modulus" of $437 \cdot 3 \times 10^6$ grams per square centimetre.

The simple rigidity of the wire was determined by the method of torsional vibrations in the manner described in a former portion of this memoir,† and proved to be $172 \cdot 3 \times 10^6$ grams per square centimetre.

Some little difficulty was experienced in finding the density of the magnesium, as when the wire was immersed in water a very large number of minute bubbles of gas—evidently a consequence of chemical action—made the apparent mass in water from 10 to 20 per cent. less than it ought to have been. Most of these bubbles could be shaken off by merely moving the wire twice or thrice backwards and forwards through the water, but in a very few seconds they collected again and rendered it evident that no reliable determination of the density could be obtained in this way. As there was not at hand any liquid which might be suitably used in place of the water, the density was ascertained from the length, diameter, and mass of three separate lengths of the wire. The diameter of each of these pieces was mea-

* "Phil. Trans.," vol. 174, 1883, pp. 61, 62.

† *Loc. cit.*, p. 24.

sured at ten different places equidistant from each other by a gauge graduated to $\frac{1}{100}$th of a millimetre, and capable of measuring by estimation to $\frac{1}{1000}$th of a millimetre. The accuracy of the gauge had been repeatedly tested on previous occasions, and could be depended upon at least to $\frac{1}{1000}$th of the diameter of the wire: nor would any error of consequence be introduced in the determinations of either the length or mass.

The following were the results:—

Number of piece.	Length in centimetres.	Mean diameter in centimetres.	Mass in grams.	Density at 20° C.
1	162·5	0·08736	1·6910	1·736
2	123·0	0·08671	1·2742	1·754
3	131·5	0·08723	1·3670	1·740
Mean		0·08710	..	1·743

The probable error in the determination of the density in this way would therefore appear to be 0·2 per cent. But values of "Young's modulus" obtained by the method of longitudinal vibrations are apt to be slightly too high in the case of wires in consequence of slight yielding of the supports at either end of the wire.* It was therefore deemed advisable to employ also the static method, and accordingly a pair of wires were suspended and examined in the manner already described in Part I of this memoir, and with the same precautions to avoid error.† "Young's modulus" as thus obtained proved to be $424·3 \times 10^6$ grams per square centimetre. It was impossible, however, in this instance to have any but a comparatively small load permanently on the wire, and in such a case the result is apt to be too low to a slight extent. We may, I think, then, find a very near approach to the true value by taking the mean between the values got by the two methods: this mean is $430·8 + 10^6$ grams per square centimetre, and is, I should say, certainly less than 1 per cent. in error.

From "Young's modulus" and the simple rigidity the ratio of lateral contraction to longitudinal extension can be calculated; this ratio would be—

$$\frac{430·8 \times 10^6}{2 \times 172·3 \times 10^6} - 1,$$

* It might be thought that any kind of yielding would depress the pitch of the wire, but the mathematical investigations of Lord Rayleigh as given in his "Theory of Sound," vol. i, p. 161, show that with *transverse* vibrations there would be in the case before us an increase of pitch. Rayleigh's investigations will equally apply, as far as the point in question is concerned, to longitudinal vibrations.

† *Loc. cit.*, pp. 2–4 inclusive.

or $$0{\cdot}2505.$$

The bulk-modulus can also be determined from "Young's modulus" and this last ratio, and is—

$$\frac{430{\cdot}8\times 10^6}{3(1-2\times 0{\cdot}2505)}$$

$$= \quad 287{\cdot}6\times 10^6 \text{ grams per square centimetre.}$$

Now it has been shown in a recent communication* to the Royal Society that the bulk-modulus can be calculated from the thermal capacity per unit volume by the formula—

$$e_v = 2071\times 10^6\times C_v^{\frac{7}{3}},$$

where e_v is the bulk-modulus and C_v the mean thermal capacity per unit volume between 0° C. and 100° C. According to Regnault the thermal capacity per unit mass is 0·2499, and according to Kopp it is 0·245.† The mean of these two numbers is 0·2480, and the thermal capacity per unit volume is therefore 0·2480 × 1·743, or 0·4323. From the above formula, therefore, we obtain—

$$e_v = 2071\times 10^6\times 0{\cdot}4{\cdot}323^{\frac{7}{3}}$$

$$= 292{\cdot}7\times 10^6.$$

This number does not differ from that obtained by observation by so much as 2 per cent., and considering the difficulties which are in the way of getting correct values of the bulk-modulus, we may regard the agreement of the observed and calculated values of e_v as sufficiently satisfactory.

Arrangement of the Magnesium Wire for Observations on the Alteration of Resistance produced by Longitudinal Traction.

Experiment VIII.

A pair of magnesium wires were arranged for an examination of the effect of longitudinal traction on the electrical resistance of the metal in the same manner as the nickel wire before mentioned, but, as it was desirable to have the permanent load as light as possible in order to avoid trespassing beyond the limits of elasticity, the scale-pan was dispensed with and the weights were suspended to a small hook‡ by string so that at the outset the permanent load on the wire only amounted to ½ kilo. When a sufficient rest had been allowed to

* "Proc. Roy. Soc.," No. 232, 1884.

† Clarke's "Constants of Nature," Smithsonian Miscellaneous Collection, No. 276, p. 15.

‡ *Loc. cit.*, p. 58, fig. 10.

enable the pair of wires to attain a constant resistance-ratio, various amounts of temporary stress from ½ to 2 kilos. were employed. The results are given below:—

Permanent load on the wire in kilos.*	Temporary load in kilos.	Temporary alteration of resistance in divisions of the platinum-iridium wire.	Temporary alteration of resistance per kilo.
½	½	4·4	8·8
½	1	10·8	10·8
½	1½	19·5	13·0
½	2	31·4	15·7
2	½	4·4	6·8

The wire completely recovered itself when each of the temporary loads was removed, and yet it will be noticed that the alteration of resistance per kilo.† varies largely with the load, so much so, indeed, that with a temporary load of 2 kilos. the temporary alteration per kilo., which is of the nature of *increase* on loading, was nearly double that when only ½ kilo. was employed for the temporary load. This marked increase of alteration of resistance per kilo. with the increase of the temporary load does not depend upon the amount of permanent stress, as we obtain the same alteration for ½ kilo. when the permanent load is ½ kilo. as we do when the permanent load is 2 kilos., but at the same time it should be observed that with the permanent load of 2 kilos. the *first* effect both of loading and unloading with ½ kilo. was considerably greater‡ than that which took place after three loadings and unloadings with the ½ kilo.§ The results given above undoubtedly point to imperfect elasticity due to the rotation of the molecules about their axes, for though the wire recovered its original resistance on the removal of the load, this is evidently due to the slight shock caused by unloading.||

Experiment IX.

In order to examine more fully the relationship which might exist between the alteration of resistance and the alteration of length caused by temporary traction, the wire used in determining the value

* Weight of clamp, hook, &c., included.

† The means of ten closely according trials with each load.

‡ More than twice as great.

§ After this the effect remained constant.

|| With some metals a marvellously small agitation suffices to make the molecules spring back to their original position after they have been permanently deflected from their positions by mechanical or other stress.

of "Young's modulus" by the method of static extension was tested with nearly the same loads, both temporary and permanent, as in the last experiment, with the following results:—

Permanent load in kilos.	Temporary load in kilos.	Temporary alteration of length in half-millimetres.	Temporary alteration of length per kilo.
½	½	2·70	5·40
½	1	5·49	5·49
½	1½	8·29	5·53
½	2	11·41	5·76
1½	½	2·37	4·74
2½	½	2·37	4·74
1	½	2·54	5·08

The numbers given in the third column are the means of several trials and, with the exception of the last two, require a slight correction, inasmuch as it was evidently impossible with such a small permanent load as ½ kilo. to obtain a sufficiently straight wire.* The correction, however, can be easily applied, for we see that when the permanent load is equal to 1½ kilos. a temporary load of ½ kilo. produces precisely the same temporary elongation as when the permanent load is 2½ kilos., or, in other words, 1½ kilos. must have sufficed to make the wire straight. Thus with a permanent load of ½ kilo. the apparent increase of length caused by the mere straightening of the wire is $2{\cdot}70-2{\cdot}37$, or 0·43 when ½ kilo. is the temporary load, and will be $0{\cdot}43+2{\cdot}53-2{\cdot}37$, or 0·59 when the temporary load is either 1 kilo. or greater than 1 kilo. Making the above corrections, and placing side by side the alteration of length and the alteration of resistance produced by each additional ½ kilo., we obtain as follows:—

Number of ½ kilos.	Temporary alteration of resistance for each successive ½ kilo.	Temporary alteration of length for each successive ½ kilo.
1	4·4	2·37
2	6·4	2·63
3	8·7	2·80
4	11·9	3·12

We may notice that though, in consequence of imperfect elasticity,†

* A matter of no importance in the electrical experiments.

† The recovery of length was as complete as the recovery of resistance had been on the removal of the stress, and this is no doubt due to the cause previously mentioned.

the temporary alteration of length increases in greater proportion than the load, this increase of effect is very much less than is the case when the alteration of resistance is concerned, the difference between the first and last numbers of the third column being only 31 per cent. as compared with the 170 per cent. difference between the first and last numbers of the second column.

It has been proved that longitudinal traction on the whole causes *increase* of resistance, but this increase is *less* than can be accounted for by mere change of dimensions, provided that the temporary stress used be sufficiently small. In the present instance it may be assumed that for any temporary stress not exceeding $\frac{1}{2}$ kilo. we shall barely enter into the region where "Hooke's law" no longer holds good,* and that up this limit of loading the increase of resistance and of length will, within the limits of errors of observation, be proportional to the load, since, were it otherwise, there could not be the accordance which there is between the value of "Young's modulus" as determined by the statical and dynamical methods.† If, then, we take 4·4 divisions of the platinum-iridium wire to represent the temporary increase of resistance produced by a load of $\frac{1}{2}$ kilo., we gather that the increase of resistance due to a stress of 1 gram per square centimetre is 1841×10^{-12} per unit. In calculating the alteration of resistance per unit, resulting from increase of length, we must take the value of "Young's modulus" obtained by using the statical method, for the alteration of resistance is necessarily determined by this method. If we do so we find that the ratio of the increase of resistance per *unit* to the increase of length per unit is 0·7813. The mere change of dimensions, however, would cause an increase of resistance of $1+25$, where 0 is the ratio of lateral contraction to longitudinal extension, and may be taken as equal to 0·2501; so that on the whole we have a *decrease* of the *specific* resistance per unit, equal to $1·5002-0·7813$, or 0·7189. With aluminium also it has been shown‡ that there is a *decrease* of *specific* resistance per unit; but this is of small amount, namely, 0·420 for the unannealed, and 0·262 for the annealed metal. We may say, then, that though magnesium does not agree with nickel and cobalt in having its resistance decreased—in spite of change of dimensions—it does agree with these metals and with aluminium in having its *specific* resistance *decreased* by longitudinal traction of small amount.

The question next arises, shall we, passing the limit of temporary stress up to which "Hooke's law" holds good, eventually find the *decrease* of specific resistance changed into *increase* when the tem-

* See *loc. cit.*, pp. 12, 13.

† The temporary elongation produced by $\frac{1}{2}$ kilo., namely, 2·37 half-millimetres, was employed in calculating the former of the two values.

‡ *Loc. cit.*, p. 52.

porary stress is comparatively large? The answer must be in the affirmative, for, as we have seen, the alteration of resistance increases with the load much more largely than the alteration of length: this is shown in the next table.

Table I.

Limits of temporary load in kilos.	Increase of resistance per unit divided by the increase of length per unit.	Alteration of specific resistance per unit divided by the increase of length per unit. + signifies increase of specific resistance.
0—½	0·7813	−0·719
½—1	1·028	−0·472
1—1½	1·307	−0·193
1½—2	1·605	+0·105

It would appear, then, at first sight, that magnesium and nickel behave in a somewhat similar manner as regards the effect of stress on their specific electrical resistance, for with nickel the decrease of resistance is changed to increase when the stress has passed a certain limit. Closer examination, however, does not justify this view, for with nickel the change from decrease to increase is conditioned by the amount of the permanent load, so that if say 6 kilos. be the limiting stress, and we maintain this stress permanently, an additional kilo. will always produce increase of resistance, though this load of 1 kilo. be taken off and put on *any number of times*, whilst with magnesium, if we regard 2 kilos. as the limiting stress, and allow this load to remain on, an additional ½ kilo. will only produce a temporary *increase* or a temporary *decrease* of specific resistance when put on and taken off respectively for *the first time*.

The Alteration of the Electrical Resistance of Platinum-iridium produced by Longitudinal Traction.

Platinum-iridium plays such an important part in the construction of standards of length and of electrical resistance, that having a length of wire made of the alloy sufficient for my purpose, I resolved to test its elasticity, and the change of electrical resistance due to stress.

Preliminary Determination of the Value of "Young's Modulus," Simple Rigidity, Density, &c.

The alloy was specially prepared for me by Messrs. Johnson and

Matthey, and an analysis made by them of a similar specimen, not, however, drawn into wire, yielded the results given below.*

	Proportion.
Platinum-iridium at 10 per cent..	99·33
Iridium in excess	0·23
Rhodium	0·18
Ruthenium	0·10
Iron	0·06
	99·90

The density at 0° C., calculated from this analysis, was 21·510, and according to MM. Deville and Mascart the coefficient of thermal expansion between 0° C. and 16° C. is 0·00002541.

As it was not considered advisable to bend the wire so as to form it into a coil suitable for finding the density in the ordinary way, the latter was calculated from the mass, length, and diameter. The length of the wire was 61·90 cm., the mass 22·088 grams, and the diameter, as measured by the gauge at seven equidistant places, was as follows :—

Number of place.	Diameter in centimetres.
1	0·1454
2	0·1454
3	0·1453
4	0·1454
5	0·1453
6	0·1450
7	0·1453
Mean	0·1453

From these data was deduced a density at 16° C. of 21·523, which value, considering the drawing to which the metal had been subjected, agrees very well with that calculated from the above analysis.

The value of "Young's modulus" was first determined by the static method in my usual way, and proved to be 2089×10^6 grams per square centimetre, and though the length of the wire operated upon was inconveniently small, the different trials agree very well with each other. The modulus was next determined by the method of longitudinal vibrations, the wire, which was sufficiently stout and rigid

* "Nature," August 7, 1879, p. 343.

for the purpose, being held by the finger and thumb in the centre, and rubbed with a resined glove. When rubbed, the wire yielded sometimes one note, and sometimes another about a semitone lower in pitch. Both these notes were fairly clear, and from the higher of the two a value for the modulus was deduced of 2276×10^6 grams per square centimetre, whilst from the lower was obtained a value which within the limits of error of observation was exactly equal to that got by the static method; so that we may regard 2089×10^6 grams per square centimetre as representing with fair accuracy* the value of "Young's modulus."

There was no difficulty in finding the value of the simple rigidity in the usual way, and this proved to be $724{\cdot}8 \times 10^6$ grams per square centimetre. From the above data the ratio of lateral contraction to longitudinal extension was calculated to be 0·441. The values of "Young's modulus" and of the simple rigidity are both high, and it is very remarkable that the 10 per cent. of iridium added to the platinum should have raised the former value more than 40 per cent.

Arrangement of the Platinum-iridium Wire for Observations on the Alteration of Resistance produced by Longitudinal Traction.

Experiment X.

We will now turn to the experiments on the alteration of resistance produced by longitudinal traction experiments, which it will be seen resulted in a complete surprise in more ways than one. In the first trials the wire was arranged in exactly the same manner as the steel and magnesium wires had been, and the lever used for applying the stress, but a German-silver wire was employed as the comparison-wire. With only a permanent stress, due to the weight of the lever, 2 kilos. put on or taken off the lever caused an alteration of resistance which required to be balanced by moving the sliding-piece 54·5 divisions, and when there was a permanent stress equivalent in all to 3 kilos. on the end of the lever, an additional kilo. on or off caused an alteration represented by 27·0 divisions, or almost exactly half the number of divisions which were required with twice the temporary load. Hence it was calculated that a stress of 1 gram per square centimetre produced an increase of resistance per unit of 3049×10^{-12}. The increase of resistance per unit divided by increase of length per unit was 6·368, and the increase of *specific* resistance per unit was 4·486. These results were very unexpected, inasmuch as all the other alloys which have been examined, namely, brass, German-silver, and platinum-silver are altered by stress in their electrical resistance far less than the pure metals of which they are composed,

* This value would seem to be correct within at least 2 per cent.

whereas here the increase of specific resistance produced by traction is nearly three times as great as with iron, which heads the list of those metals which show increase of resistance on loading.

Experiment XI.

Under the above-mentioned circumstances, it was deemed to be advisable to test the platinum-iridium in another manner. In this case, instead of using German-silver as the comparison-wire, the following arrangement was made:—

To the centre of the wire was firmly fixed a brass clamp, which rested on a stout table, and was connected with one terminal of the galvanometer, the other terminal being as usual connected with the sliding-piece. The lower half of the wire, which passed through a small hole in the table, was stretched by means of the lever, whilst the upper half remained unstretched. Near the extremities of both halves were fixed other clamps, provided with terminal screws, which in each case were united as usual with one pole of a battery, and with a set of resistance coils joined to each other by the platinum-iridium wire traversed by the sliding-piece. With this arrangement it will be seen that the upper half of the wire served as a comparison-wire to the lower half. The results of this experiment were in fair accordance with those of the previous one, and still showed platinum-iridium to be considerably more increased in resistance by traction than any of the other metals.

Experiment XII.

In order to remove any further doubt about clamping not being sufficient to make proper connexion between the various parts of the "bridge," a third experiment was tried, in which the platinum-iridium wire, with a German-silver wire as a comparison-wire, were arranged as in Experiment X, but now all connexions with the wire to be stretched were well soldered, and a long series of trials, extending over three days, was made with a view to not only confirm the results of Experiments X and XI, but also to bring out any fresh peculiarities which might exist. The main points to which it is well to call attention are shown in the last two series of trials given below, in which the permanent stress was that due to the weight of the lever itself.

It is noteworthy here that the resistance at first increases in greater proportion than the load, but when a certain limit of stress has been reached the ratio of the increase of resistance to the load producing it begins to diminish, until finally the last kilogram only produces the same alteration of resistance as the first. A similar state of things was perceived with wires made of other metals in the experiments

Kilos. on the lever.	Temporary alteration of resistance in terms of the divisions of the *graduated* platinum-iridium wire. First series.	Temporary alteration of resistance, &c. Second series.	Mean alteration from the two series.
1st kilo.	23·0	23·5	23·25
2nd ,,	23·5	24·5	24·00
3rd ,,	29·5	29·5	29·50
4th ,,	32·0	33·5	32·75
5th ,,	35·0	29·0	32·00
6th ,,	27·0	28·0	27·50
7th ,,	23·0	26·5	24·75
8th ,,	..	23·5	23·50

described in the previous portions of this memoir,* and, moreover, there is a like change in the elasticity,† but with platinum-iridium the effect, as far as alteration of the resistance is concerned, is more marked than is the case with other metals. Equally noteworthy is it that at the third kilogram there is a *sudden* increase in the temporary alteration of resistance produced by the load, and a like phenomenon is plainly discernible in the above-mentioned previous experiments. Moreover, the stress which produces this sudden increase is certainly not far from the stress which produces the first sudden leap in the value of the ratio of the *permanent* alteration of resistance to the load, when the annealed wire is stretched for the first time;‡ nay, more, a careful examination of Experiment VII§ shows undoubted evidence of the existence of the same number of critical points where *temporary* alteration is concerned, as there are in the case of *permanent* extension.

Finally, it must be added, that if we take the mean effect produced by all the loads on the resistance of the wire, we obtain an increase of resistance which is exactly equal to that already recorded in Experiment X, and therefore we must regard the comparatively large increase of resistance of platinum-iridium caused by traction as a well-established fact.||

* *Loc. cit.*, p. 50, where by taking the alteration of resistance caused by consecutive loads of 2 kilos. each, this can be plainly discerned.

† *Loc. cit.*, p. 16.

‡ Compare Experiments VII and XXIII, *loc. cit.*, pp. 50, 82. As regards Experiment XXIII, it should be remarked that more recent observations have shown that there is a critical point at the third kilogram (fifth kilogram if we include the weight of the scale-pan itself), and that here therefore we must look for the first critical point and not at the eighth kilogram (tenth kilogram including the weight of the scale-pan), where is the *second* critical point.

§ *Loc. cit.*, p. 50.

|| This comparatively large increase of resistance produced by traction is rather against the use of platinum-iridium in the construction of standard resistance coils.

The Effect of Longitudinal Traction on the Thermo-electric Properties of Platinum-iridium.

Experiment XIII.

The above-mentioned large increase of resistance resulting from the temporary traction of platinum-iridium, together with the apparent relationship between the effect of mechanical stress on the thermo-electric properties of metals, and that on the specific resistance, rendered it probable that stress would act on the thermo-electric properties of iron and platinum-iridium similarly as regards nature, but with greater intensity with the latter metal than with the former, but here, again, a surprise was met with, for when the wire was tested in the same manner as the cobalt had been, the *unstretched* platinum-iridium was found to be *positive* to the *temporarily stretched* metal, and therefore the alloy is affected by stress, thermo-electrically, in a manner similar to *nickel.* Thus we see that, though these new experiments would in the case of cobalt and magnesium largely confirm us in our opinion respecting the above-mentioned relationship, it is quite the contrary in the case of platinum-iridium. In the next table will be found drawn up the results obtained by Hall, Bidwell, and myself.

Hall says of the numbers in the second column: "I cannot vouch for the quantities within 50 per cent., but I think I can vouch for the direction of the effect." Bearing this statement in mind, and the difficulties which lie in the way of obtaining accurate values for the ratio of lateral contraction to longitudinal extension, which difficulties will affect the numbers in the fourth and fifth columns, one cannot help being struck with the fact that with most of the metals the *order* of the "rotational coefficients" is the same as that of the alteration of specific resistance caused by traction. Cobalt and platinum are, however, conspicuous exceptions, but with regard to the former metal it has been already observed, and of course the same observation would apply equally to nickel and iron, that longitudinal traction might produce different effects in the magnetised and unmagnetised metals. The exception furnished by platinum cannot be thus accounted for, nor does it seem fair to attribute the discrepancy either to errors of observation or to difference in the purity of the specimens examined by Hall and myself respectively.* In the fifth column is given the difference between the alteration of the specific resistance of lead by traction, and that of the other metals, and, with the exceptions just mentioned,† it may be fairly said that, within the errors of

* The specimen of platinum used by myself was obtained from Messrs. Johnson and Matthey as chemically pure.

† I ought to remark here that according to Bidwell the "Hall effect" in aluminum is +. In a trial made by myself on the pure specimens of aluminium in

Table II.

Metal.	"Rotational coefficient."*	Direction of thermo-electric current.†	Alteration of specific resistance per unit divided by increase of length per unit.‡	Ditto. −1·613
Iron	+ 78·0	+	+2·618	+ 1·005
Steel	..	+	+2·082	+ 0·469
Cobalt	+ 25·0	−	−2·203	− 3·816
Zinc	+ 15·0	+	+2·113	+ 0·500
Lead	0	0	+1·613	0
Tin	− 0·2	−	+1·630	+ 0·017
Brass	− 1·3	−	+0·166	− 1·447
Platinum	− 2·4	−	+2·239	+ 1·626
Gold	− 6·8	−		
Silver	− 8·6	−	+1·617	+ 0·004
Copper	− 10·0	−	+1·005	− 0·382
Aluminum	− 50·0	+	−0·420	− 2·033
Magnesium	− 50·0	−	−0·733	− 2·346
Nickel	−120·0	−	−8·860	−10·473
German-silver	—	−	+0·226	− 1·387
Platinum-silver	—	−	+0·624	− 0·989
Platinum-iridium	—	−	+4·486	+ 2·873

observation to which the numbers in this column are liable to be affected, the signs are here the same as those in the second column.

As regards the relationship between the alteration of specific resistance by traction and that of the thermo-electric qualities of the metals due to the same cause, it may be seen by comparing the third and fifth columns, that out of sixteen different metals, pure and alloyed, the signs in the two columns are decidedly the same in eleven instances, are decidedly different in three instances, and are dubiously so in two instances.

It has been noticed§ that if the density of a substance be denoted by Δ, and A represent the atomic mass, so that the difference between the centres of adjacent molecules be proportional to $\left(\frac{A}{\Delta}\right)^{\frac{1}{3}} = x$, say,

my possession, the effect of traction on the thermo-electric properties of the metal seemed to be the same in direction as that given, and for which Bidwell is my authority, but the effect was very slight, and I am not now quite certain whether a less stress than was then applied would not give an opposite result.

* A + sign in this column signifies that the effect is in a direction the same as that which the conductor itself bearing the current would follow if free to move across the lines of magnetic force (see "Nature," Nov. 10, 1881, p. 46).

† A + sign in this column signifies that the direction of the thermo-electric current is from *unstretched* to *stretched* through the *hot junction*.

‡ A + sign in this column signifies *increase* of resistance.

§ *Loc. cit.*, p. 32.

$e \times \alpha^7$ is, with most metals, a constant, where e denotes "Young's modulus." Taking the mean of the values of e and of Δ for cobalt in the annealed and unannealed conditions, we have $\alpha = 1{\cdot}927$, and $e \times \alpha^7 = 1886 \times 10^8$. For magnesium $\alpha = 2{\cdot}399$, and $e \times \alpha^7 = 1969 \times 10^8$.

In the next table will be found collected together most of those results of the present inquiry which can be expressed by numbers.

It has been suggested with regard to the method of finding the effect of strain on the resistance, that unless the strain was fairly uniform, thermo-electric effect might have to some extent vitiated the results. It will be seen, however, that the strain must have been fairly uniform, and even if there had been considerably more lack of uniformity than actually existed, the small change which can be wrought by stress and strain in the thermo-electric power of any of the metals hitherto used, would preclude the possibility of any appreciable vitiation. Further, in many cases, very different lengths of the same wire were tested and found to yield the same results, which would not have been the case had the strain on the connexions, a point also suggested for consideration, introduced error.

Summary.

1. The electrical resistance of cobalt, like that of nickel, is temporarily decreased by temporary longitudinal traction. Whether the decrease of resistance would be changed to increase, as it is with nickel, by a greater amount of stress, has not yet been ascertained, but should this be the case, the magnitude of the stress per unit area which would suffice for the purpose, must be much greater with cobalt than with nickel.

2. Permanent extension and rolling diminishes with cobalt as with nickel, the effect of longitudinal traction alluded to in 1.

3. Cobalt is remarkable for the extreme persistence with which the same load, when applied again and again, continues to produce permanent increase of resistance.

4. Moderate permanent extension decreases permanently with cobalt as with nickel and iron, the *specific* resistance.

5. Temporary longitudinal traction renders cobalt temporarily positive as regards its thermo-electrical qualities to cobalt not under traction, provided there is no magnetic stress acting at the same time.

6. Temporary longitudinal magnetic stress renders cobalt temporarily negative as regards its thermo-electrical qualities to cobalt not under magnetic stress, provided there is no mechanical stress acting at the same time.

7. The effect of temporary longitudinal traction, even when carried to very great excess, is to increase the resistance of unannealed piano-steel; and this increase, though less than with iron, both for unit

Table III.

Metal.	Condition.	Density.	"Young's modulus" in grams per sq. cm.	"Simple rigidity" in grams per sq. cm.	Specific resistance, *i.e.*, resistance in ohms of 1 c.c. between opposing forces.	Alteration of resistance per unit produced by a longitudinal stress of 1 grm. per sq. cm.*	Alteration of resistance per unit divided by increase of length per unit.	Alteration of specific resistance per unit divided by increase of length per unit.
Cobalt	Unannealed.	8·231	2005×10^{6}	..	2450×10^{-8}	$-\ 242 \cdot 3 \times 10^{-12}$	−0·486	−1·986
Cobalt	Annealed...	8·259	1817	..	2289	− 386·8	−0·703	−2·203
Magnesium .	Unannealed .	1·743	430·8	$172 \cdot 3 \times 10^{6}$	565	+1841	+0·781	−0·719
Platinum-iridium	Unannealed .	21·523	2089	724·8	2830	+3049	+6·368	+4·486

* A + sign in this and the next two columns signifies *increase* of resistance on the *application* of stress.

stress per square unit of area and per unit temporary increase of length, is much greater than can be accounted for by changes in the dimensions of the steel.

8. The electrical resistance of magnesium is temporarily increased by temporary longitudinal traction of moderate amount, but the amount of increase is less than can be accounted for by mere change of dimensions, so that the *specific* resistance of magnesium, like that of aluminium, is diminished by the temporary stress.

9. *When the permanent load on the wire is very small*, the temporary increase of length, like that of the increase of resistance, increases in larger proportion than the temporary load, but the former increases less rapidly than the latter, so that when the temporary stress exceeds a certain limit the above-mentioned decrease of specific resistance is changed to an increase of specific resistance.

10. The values of "Young's modulus," and the simple rigidity of the alloy, platinum-iridium, are much greater than those calculated from the same values for the components of the alloy.

11. The electrical resistance of platinum-iridium, quite unlike that of platinum-silver, German-silver, and brass, is much more increased by temporary longitudinal traction than that of either of the components of the alloy.

12. The increase of resistance mentioned above is much greater than can be accounted for by change of dimensions, so that the increase of *specific* resistance produced by longitudinal traction is considerably greater than is the case with any of those other metals examined whose resistance is increased by longitudinal traction.

13. The alteration of the resistance alluded to in 11 at first increases in greater proportion than the load, but when a certain limit of stress has been reached, the ratio of the temporary increase of resistance to the load producing it, begins to diminish, and finally reaches the same value as at first. A tendency to a similar state of things is seen with other metals, but in none is the phenomenon so pronounced as in platinum-iridium.

14. Unstretched platinum-iridium is thermo-electrically positive to the temporarily stretched metal.

15. The present investigations, as far as magnesium is concerned, confirm the previous ones in showing an apparent relationship between the "Hall effect" and the alteration of the specific resistance produced by mechanical stress. With regard to cobalt this is not so, at any rate for the metal when not under the influence of magnetic stress.

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OBITUARY NOTICES OF FELLOWS DECEASED.

Amongst the men who have laboured earnestly and successfully to place on a sound scientific basis the practice of engineering, the late accomplished occupant of the Chair of Engineering at the University of Edinburgh, HENRY CHARLES FLEEMING JENKIN, will hold a distinguished place. Born in Kent on the 25th March, 1833, the only son of Captain Charles Jenkin, R.N., he was sent to school in Scotland at the early age of seven years, where, under Dr. Burnett, of Jedburgh, for three years, and after that for three years in the Edinburgh Academy, the first six years of his school life were spent. In 1846 he was placed at a school in Frankfort; in 1847 he was for a time in Paris; and, finally, in 1850, he graduated as a Master of Arts at the University of Genoa.

He began his training as an engineer in a locomotive workshop at Genoa, under Philip Taylor, of Marseilles, where he remained for about a year. He returned to England in 1851, and served a three years' apprenticeship in the works of the Fairbairns, of Manchester. After a varied experience of practical work, Mr. Jenkin, in 1857, entered the service of Messrs. Newall, in their submarine cable factory at Birkenhead, where they were then engaged in the manufacture of a part of the first Atlantic cable, and afterwards of cables for the Mediterranean and the Red Sea. His energy and talents very soon obtained for him the position of chief of the engineering and electrical staff. In this connexion Jenkin was brought into close relation with the able engineers and electricians who were then working out to a practical result the great problem of submarine telegraphy. These circumstances determined the direction in which his energies were more especially to be applied, and he became early known as an electrical engineer of high standing.

At the beginning of 1859 he became known to Sir William Thomson, and entered into constant correspondence with him in connexion with the testing of conductivity and insulation of submarine cables, and the speed of signalling through them. After Faraday's discovery of the *existence of specific inductive capacity*, and his now celebrated, though then ignored, determinations of it for flint glass, shell-lac, and sulphur, the first correct determination of the specific inductive capacity of any substance was made by Jenkin by means of observations arranged for the purpose on some of the submarine cables in the factory at Birkenhead.

In 1861 Mr. Jenkin joined Mr. H. C. Ford as partner, and with him for seven years he carried on an extensive practice in telegraphic and general engineering. During the last two years of this partnership Jenkin held the post of Professor of Engineering at University College, London, and in 1868 the partnership was dissolved on account of his appointment to fill the Chair of Engineering in the University of Edinburgh, which he occupied till his death, teaching with much success.

In 1859 he began to write upon scientific subjects, encouraged to do so, as he has himself remarked, by Sir William Thomson. His published papers are in all about forty in number. Of these a large proportion deal with questions arising from the science and practice of submarine electrical engineering, and were published within the ten years 1859 to 1869—a period of the greatest progress in submarine telegraphy.

Professor Fleeming Jenkin took a very important part in the work of the British Association Committee on Electrical Standards, appointed on the suggestion of Sir William Thomson at the Manchester meeting of 1861, for the purpose of promoting the practical use of Gauss and Weber's system of absolute measurement, by which lasting benefit has been conferred on electric and magnetic science. Jenkin was made Secretary of this Committee; and, in conjunction with Professor Clerk Maxwell, carried out the most important of the experiments instituted by the Committee.

Through having been so intimately concerned in the beginnings of ocean telegraphy, Jenkin became associated with Sir William Thomson and Mr. C. F. Varley in the development of the instruments by which the transmission of messages over long submarine cables was for the first time made practicable. During later years he and Sir William Thomson acted as joint engineers for various cable companies, their latest work in that capacity being the Atlantic and other cables of the Commercial Cable Company.

For the last two years he was much occupied with a new mode of electric locomotion, a very remarkable invention of his own, to which he gave the name of "Telpherage." He persevered with endless ingenuity in carrying out the numerous and difficult mechanical arrangements essential to the project, up to the very last days of his work in life. He had completed almost every detail of the realisation of the system which was recently opened for practical working at Glynde, in Sussex, four months after his death.

His book on "Magnetism and Electricity," published as one of Longman's elementary series in 1873, marked a new departure in the exposition of electricity, as the first text-book containing a systematic application of the quantitative methods inaugurated by the British Association Committee on Electrical Standards. In 1883 the seventh

edition was published, after there had already appeared two foreign editions, one in Italian and the other in German.

His papers on purely engineering subjects, though not numerous, are interesting and valuable. Amongst these may be mentioned the article "Bridges," written by him for the ninth edition of the "Encyclopædia Britannica," and afterwards republished as a separate treatise in 1876; and a paper "On the Practical Application of Reciprocal Figures to the Calculation of Strains in Framework," read before the Royal Society of Edinburgh, and published in the "Transactions" of that Society in 1869. But perhaps the most important of all is his paper "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery," read before the Royal Society of Edinburgh, and published in the "Transactions," vol. xxviii (1876–78), for which he was awarded the Keith Gold Medal. This paper was a continuation of the subject treated in "Reulaux's Mechanism," and, recognising the value of that work, supplied the elements required to constitute from Reulaux's kinematic system a full machine receiving energy and doing work.

Professor Jenkin's activity was not, however, confined to purely scientific pursuits. The very important practical subject of healthy houses largely engaged his attention during the last eight or ten years of his life, and he succeeded so well in impressing its importance on public opinion, that he obtained the establishment in many large towns of Sanitary Protection Associations. He also took great interest in technical education, and was always ready in word and deed to aid in its promotion. His literary abilities were of no mean order, and as a critic he made several marked successes, among which his reviews of Darwin's "Origin of Species" and of Munro's "Lucretius" (the atomic theory) may be referred to as of high scientific merit.

He was elected a Fellow of this Society in 1865; he was also a Vice-President of the Royal Society of Edinburgh, a Member of the Institution of Civil Engineers, and of the Institution of Mechanical Engineers, and in 1883 he received the honorary degree of LL.D from the University of Glasgow. He died on the 12th of June, 1885, after a few days' illness, due to a slight surgical operation. W. T.

F. G. J. Henle. Of the great anatomists and physiologists whose discoveries made the middle of the present century a never-to-be-forgotten epoch in the history of biology, one of the greatest died on the 13th of last May at Göttingen. Frederick Gustavus Jacob Henle was, in the earlier years of his career, the most distinguished of living pathologists, and, indeed, founded the Science of Pathology as we now understand it. The "Manual of Rational Pathology," published in 1846, was the first important work in which the observed clinical and anatomical facts of disease were classified and brought

together into a system, based on their physiological relations. He had already prepared the way for his reform of Pathology by his larger work on General Anatomy, in which he, for the first time, distinguished the tissues which make up the framework of the body from each other, according to their chemical and anatomical characters, availing himself, as regards the latter, of the new light which the discoveries of Schwann had thrown upon the nature of animal and plant organisation, and of the methods of investigation rendered possible by the sudden advance which had taken place in the construction of the microscope.

As a pathologist, Henle soon relinquished the lead he had at first taken to Virchow, who, by his personal work or by that of his pupils, has since retained it. As years went on, Henle became more and more a descriptive anatomist and histologist. The publication of the greatest work of his incredibly laborious life, the "Handbuch der Systematischen Anatomie des Menschen," began in 1855. In it the student was furnished with an anatomical description of the human body, which, at the time, surpassed all others—not even excepting Dr. Sharpey's edition of Quain's Anatomy—in the quantity of information, new and old, it contained, in the abundance and excellence of the illustrations; and it was not less remarkable for the clearness of the author's style, and the power which he possessed of so presenting the forms and anatomical relations of the organs or parts described, as to leave behind a vivid picture in the imagination of the reader—for Henle's descriptions are so real and true to nature, that in reading them one seems to have seen what is described.

Any one of the three great works which have been mentioned would have been sufficient to entitle its author to a permanent position among the founders of modern medical science. Taken together they afford good ground for assigning to Henle a place scarcely inferior to that of his master, Johannes Müller, or of his contemporary, Theodore Schwann.

Henle began his career as an anatomical investigator by spending the year which intervened between his examination for the doctorate and the taking of his degree in the preparation of his thesis on the Anatomy of the Eye, and in other anatomical researches, of which the fruits appeared later, and which he undertook, under the direction and with the co-operation of J. Müller, who was still at Bonn. When Müller, a year later, was called to the University of Berlin, Henle became his Prosector. He held this office for six years, during which he was associated with Müller in his multifarious professorial duties; these comprising, according to the custom of the time, not only the teaching of anatomy and physiology, but also pathology and pathological anatomy. It was for Henle a time of extraordinary activity. During his tenure of this Prosectorship he published three

anatomical monographs on previously undescribed species of animals (Narcine, Bronchiobdella, Enchytrœus), one or two contributions to descriptive human anatomy, and (in addition to the Latin dissertation already referred to) four of those investigations into the minute structure of the animal body, which afford the best justification for placing him, as we have done, side by side with Theodore Schwann, as one of the founders of the Science of Histology—namely, the Essay on the Structure of the Lacteal System, which served for his "Habilitation" Thesis, and three other papers on the Distribution of the Epithelium in the Human Body, on the Formation of Mucus and Pus, and on the Structure and Development of Human Hair.

In 1840, when Henle was thirty years old, he received a call to the Chair of Anatomy at Zürich, where he remained until, in 1844, he became the colleague of Tiedemann, at Heidelberg. The year after his Zürich appointment he published his "General Anatomy," already referred to; and about the same time, in co-operation with Professor J. Müller, he published a zoological work on Sharks and Rays, for which the collection of the British Museum afforded part, at all events, of the material.

In Heidelberg, Henle, like Müller, taught both anatomy and physiology, as well as pathology. Almost immediately after his settlement in that university, he began with Pfeiffer, who had accompanied him from Zürich, the "Zeitschrift für rationelle Medicin," which, after sharing the first place with "Virchow's Archiv" in medical periodical literature for a quarter of a century, gave place to the new order in 1872. During the last sixteen years of its existence the Journal was enriched by the publication in it of Henle's own annual account of the progress of anatomical and physiological science—a fact which, irrespectively of its other contents, will give its fifty-four volumes a permanent place in medical literature. As all who were then engaged in the study of biology know, Henle's "Berichte" had no analogy to the half reliable and unappreciative abstracts which the enormous growth of periodical literature have now made a necessary evil. They consisted rather of records by a master hand of all that was worthy of being remembered, and critical reviews of all that was worthy of being discussed in its bearing on the development of the branches of science to which they related. So that whoever undertakes the Herculean task of writing the history of that time of rapid progress in biology which began about the time of Henle's removal from Berlin to Zürich, will find the chief events of those thirty years continuously chronicled (first in "Müller's Archiv," then in "Canstalt's Jahresberichte," and finally, as above stated, in the "Zeitschrift") by one who himself took a prominent part in them.

In 1852 Henle was called from Heidelberg to Göttingen, and it was here that the chief work of his life, the preparation of the Descriptive

Anatomy, was accomplished. It resembles the contemporary edition of Quain and Sharpey in including both general and special anatomy, and in the way in which the illustrations are interwoven with the text; but exceeded it in their number and quantity. To the most difficult part of descriptive anatomy, that which relates to the central organs of the nervous system and to the viscera, Henle devoted many years of labour, and produced a result which had never before been approached. The *magnum opus* was brought to a conclusion in 1873, but inasmuch as by the time the first edition was completed, the third was already in progress, it continued to the end to be an increasing occupation, the labour of which was further enhanced by the obligation to which the requirements of medical education forced upon him, of publishing a text-book and copious atlas for students—a compendium of the larger work.

The "Handbook" did not of course profess to contain a record of researches. It was in its end and construction a system. But during the whole course of its publication, the author was continuously engaged in the work of investigation. The research on the anatomy of the kidneys, in connexion with which subject his name is most familiar to English students of medicine, appeared in 1862 in the Transactions of the Göttingen Academy; those on the histology of the central nervous system in 1867–68. More recently he published two important researches on his old subject, the minute anatomy of the eye (1878 and 1882); and finally in November, 1884, his last anatomical research, "Das Wachsthum des Menschlichen Nagel und des Pferdehufes."

To judge of Henle as a pathologist, reference must be made to his systematic work on that subject already mentioned, the fundamental idea of which is, that disease consists essentially in the reaction of the living material against "abnormal external action," and that the nature of this reaction differs in no respect, excepting the circumstances under which it is evoked or induced, from those which exhibit themselves in the healthy body in its relation to its normal environment, so that, as Henle expressed it, "physiology and pathology are branches of the same science." The notion expressed in these words, incontrovertible as it may appear now, was opposed to the teaching of the time, which in physiology preferred to inquire into the purpose rather than the cause of vital phenomena, and resented as a desecration every attempt to refer them to chemical or physical actions; and in pathology spoke of diseases as if they were mischievous personalities whose intentions it was the business of the physician to aid the "Schützender Geist" in discovering and frustrating. Against all such notions Henle made uncompromising war, by showing even with the imperfect knowledge and means possessed in 1840, that it was possible to discover the causes of many diseases, not in intestine

strivings between rival destructive and preservative tendencies supposed to be resident in the body itself, but in the existence in its environment of elements injurious to its welfare.

At an earlier period, the absence of physical and chemical knowledge would have rendered the success of any such attempt impossible. As it was it was imperfect, and was recognised as such by its author; but it was so unquestionably the beginning of that rapid development, by which, under the influence of Virchow and his pupils, what we now recognise as the science of pathology has come into existence, that of this science Henle must be regarded as the founder.

In certain directions, indeed, Henle was in advance of his successors. If, for example, we take the opening chapter of the "Untersuchungen," which deals with the etiology of contagious diseases, we find in it a most remarkable anticipation of the discoveries in this field of inquiry of the last fifteen years. He sets forth in the clearest language that the material of contagium must necessarily be not only organic but living and organised, and that it must consist of "parasitical beings which are certainly among the lowliest, smallest, but at the same time most productive which are known."

The consideration of the reasoning which led Henle to this conclusion, affords a striking illustration of the way in which discoveries made in other departments of natural science influence medicine. Henle's grounds were* (1) the evidence of experiments that contagion acts in infinitesimal quantities, and must therefore be self-multiplying; (2) the proof then shortly before given by Schwann that the analogons processes of fermentation and putrefaction are dependent on minute organisms; (3) the proof recently given by Bassett Audouin that the muscardin of silkworms, a disease communicable through the air, was due to a *contagium vivum;* and finally (4), the consideration that the development of contagious diseases could be best explained by attributing them to a living cause.

That Henle did not himself follow these indications may probably be attributed to his being engrossed by other researches. It seems at first sight difficult to account for the fact that the seed sown by him did not fructify in the minds of some of his readers; for when in 1868 the investigations of Chauveau again brought the subject of *contagium vivum* to the front, it was approached from an entirely different point of view.

A sketch must be given, in conclusion, of the views which Henle entertained and taught as to the psychological side of biology. Mention has already been made of his having lectured at Heidelberg on anthropology. These lectures were given to a mixed academical

* See his "Pathologische Untersuchungen," Berlin, 1840, pp. 17—20, and 36—41. The publication of this work was preparatory to that of the "Rationelle Pathologie."

audience in 1847–52, and published thirty years afterwards under the title of "Anthropologische Vorträge." That they dealt with subjects very far apart from what we now understand by anthropology may be gathered from the titles—Faith and Materialism; Taste and Conscience; The Physiology of Emotion; The Will; Teleology and Darwinism; Medical and Religious Dualism; &c. Among the most interesting are the two last enumerated. Like many other biologists of the former generation, Henle, while cordially accepting the doctrine of descent, strenuously opposed the monistic view, which in the minds of most persons is associated with it, and which refuses to find anything in the phenomena of life which cannot be accounted for as resulting from the play of the molecular forces of the chemical elements which take part in them. He regarded the organism, whether plant or animal, not as the inevitable product of the conditions under which it originated and was developed, but as having independent powers of its own, which the environment is capable of modifying or even controlling, but not of originating. The constancy as well as the variability of organic structure, he said, are alike manifestations of the existence of an *agent* attached to matter, but not material, and endowed with the function of "presiding over the metabolism of the body capable of reproducing the typical form, and of endless partition without diminution of intensity," in a way which has no counterpart in the inorganic world. It is this infinity of the faculty which the organism possesses of "making the material which composes it its own, and impressing upon it its stamp," which separates it from the recognised forces of inorganic nature. Every kind of organism has its "räumliches" as well as its "zeitliches Ziel,"* which serves as the law of its existence, and the fulfilment of which in no way interferes with its taking its legitimate part in the order of nature. J. B. S.

THOMAS DAVIDSON, LL.D., whose death took place at his residence in Brighton on Wednesday, October 14th, was born in Edinburgh, May 17th, 1817; his ancestral home being at Muir House, near Edinburgh. Mr. Davidson's family possessed considerable landed property in the county of Midlothian.

At the early age of six years he was taken to the Continent and entirely educated in France, Italy, and Switzerland under the tuition of French and Italian masters. Even at eleven years of age he exhibited a marked predilection for natural history, as well as the fine arts, especially painting, and every facility was afforded him to secure the great advantage which Paris then offered to the artistic and scientific student.

*"Teleologie und Darwinismus," p. 92.

Young Mr. Davidson attended the courses of lectures delivered at the Sorbonne, Jardin des Plants, École des Mines, and Collége de France. These courses were given by Cordier, Elie de Beaumont, Constant Prevost, Dufrenoy, Geoffroy Saint Hilaire, Dumeril, Valenceinnes, de Blainville, Milne Edwards, Audouin, Brongniart, Pouillet, &c.

In 1832 Sir Charles Lyell's "Principles of Geology," and his intimacy with Constant Prevost, led him to give much attention to geology and palæontology, and at fifteen years of age he had already, under the guidance of Prevost, explored the greater part of the Paris basin, securing a good collection of its rocks and typical fossils.

In 1835 he matriculated at the University of Edinburgh, studied mineralogy under Professor Jamieson, chemistry under Dr. Reid, and assisted Mr. R. Cunningham in his geological survey of the Lothians.

In 1836 he returned to the Continent and explored a considerable portion of France, Belgium, Switzerland, Germany, and Italy; his acquaintance in 1837 with the distinguished Prussian geologist, Von Buch, led to his undertaking the special study of the then little understood recent and fossil Brachiopoda, the elaboration of which or any other group of Mollusca has never been surpassed; the elucidation of their characters, classification, and history, as well as of their geological and geographical distribution, being now complete through his labours. From that year up to a short period prior to his death, Mr. Davidson unceasingly laboured to advance this special branch of Palæontology.

For some years Mr. Davidson was an attentive and distinguished pupil of Paul Delaroche, and studied under Horace Vernet and other French Academicians at the École des Beaux Arts. He spent the winter of 1841 in Rome, devoting himself to the art of painting, but his love for scientific research predominated, and he subsequently brought his artistic knowledge to bear upon his favourite scientific branch of study.

In 1846–47 Mr. Davidson made a careful examination of the Silurian districts for the purpose of his palæontological researches, and in 1850 he commenced that grand series of memoirs in the "Palæontographical Society's Transactions," which mainly terminated in 1871 (extensive supplements following). This distinguished naturalist has published or written for the Palæontographical Society five large quarto volumes, containing nearly 3000 pages of text, and 250 plates, all the figures of which have been executed by himself and presented to the Society free of all expense, and this on one subject only.*

* The monographs extend from the year 1851 to the present time, the final portion of the fifth volume being now in the press, and completing his great and laborious life-work. His posthumous papers will shortly be published in the "Transactions of the Linnean Society," his last contribution being a complete and finely illustrated monograph upon the "Recent Brachiopoda," in three parts.

Mr. Davidson prepared the article Brachiopoda for the "Encyclopedia Britannica," and has monographed the entire series of Brachiopoda collected during the exploration of H.M. ship "Challenger."

Thirty years have passed since the publication of his general introduction to the *first* volume of his extensive monographs, and well may it be said that seldom has fortune more completely equipped a student for his life-work than in the present case, in which more than ordinary artistic talent, liberal education, and independent means were joined to unsurpassed devotion in the pursuit of knowledge, with just impartiality in the recognition of the labours of others in the same field of research. Mr. Davidson was one of the most unselfish of men, ready to aid all who worked and sought his advice and opinion upon the intricacies of the intimate structure of the Brachiopoda. Few more enthusiastic students ever enriched the pages of scientific literature, or probably committed fewer errors in the description and delineation of the voluminous mass of species that enrich the 3000 pages and 250 plates left as a lasting memorial of industry and learning. Unhappily Mr. Davidson has not lived to see the proofs of his final monograph on the recent species of Brachiopoda about to be issued by the Linnean Society.*

Beyond the monographs in the palæontographical volumes, most of Mr. Davidson's investigations have been published in the "Quarterly Journal of the Geological Society," the "Bulletin de la Société Géologique de France," the "Annals and Magazine of Natural History," the "Geological Magazine," "Proceedings" of the Linnean Society of Normandy and of the Zoological Society of London, "Transactions" of the Geological Society of Glasgow and of the Royal Society of Liège.

In 1880 Mr. Davidson was deputed by the President and Council of the Geological Society of London to represent the Society at the celebration of the fiftieth Anniversary of the foundation of the French Geological Society; he was received in Paris with great distinction. The Councils of the Royal Society of London, the Linnean Society, the Zoological Society, the Royal Society of Edinburgh, the Geological Society of Edinburgh, the Geological Society of Glasgow, the Royal Academy of Ireland, the Royal Geological Society of Ireland, and the Palæontological Society of London all commissioned Mr. Davidson

* Less than one-third of the known species (1000) of Brachiopoda were unknown when Mr. Davidson published the introduction to his great work in 1850. To this volume the late Dr. W. B. Carpenter appended an elaborate description of the "Intimate Structure of the Shells of Brachiopoda," and Sir Richard Owen a learned contribution on the "Anatomy of Terebratula" The number of British species, which in 1850 comprised 13 genera and 450 species, have now expanded to 74 genera and 1000 species and varieties, these results being almost entirely due to the researches of Mr. Davidson.

to felicitate the Council of the Geological Society of France on its prosperous condition on attaining its fiftieth Anniversary.

Mr. Davidson was member or honorary member of most of the distinguished societies of Europe and America. He was elected Fellow of the Royal Society in 1857, was Fellow of the Linnean Society, the Geological Societies of London, France, Edinburgh, and Glasgow, Vice-President of the Palæontographical Society, Member Étranger de l'Institute des Provinces, France, and Linnean Society of Normandy, the Imperial Society of Naturalists of Moscow, Imperial Mineralogical Society of St. Petersburgh, Member of the Royal Academies of Belgium and Bavaria, Société Royale Hollandaise des Sciences, Haarlem, Royal Society of Liège, Academy of St. Louis, the American Philosophical Society, Philadelphia, Palæontological Societies of Belgium and Switzerland, the Zoological Society of Vienna, and many of the local societies of Britain.

In December, 1870, he was awarded one of the Royal medals in recognition of his valuable contributions to palæontology.

In July, 1865, he received from the Council of the Geological Society the Wollaston medal, its highest award for distinguished merit.

In 1882 the University of St. Andrews conferred on him the honorary degree of LL.D.

The Free Library and Museum at Brighton chiefly owed its foundation and success to Mr. Davidson's energy and perseverance. He was permanent chairman of the Museum Committee at the time of his death. Mr. Davidson has generously bequeathed to the nation his magnificent and unique collection of recent and fossil Brachiopoda largely enriched with types, together with his fine collection of books and original drawings. These will all be preserved in the Department of Palæontology in the British Museum of Natural History, Cromwell Road, South Kensington. R. E.